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ART. I.—*General View of the Origin, Characters, and Disposition of Unstratified Rocks and Veins.* By J. Mac Culloch, M.D., F.R.S.

THE unstratified rocks are far less numerous than the stratified, and they may be comprised under two general heads. Granite forms one of these, and to the other the term Trap has been applied, with some latitude it must be owned; but to avoid the necessity of inventing a new one, I shall here use it even for porphyry, when a general term is required, as it is often very inconvenient, when treating of general phenomena, to shift from one word to the other. With respect to the various kinds of rock implied under the general term Trap, I must refer to my work on the Classification of Rocks, as it would be mere repetition to introduce that part of the subject in this place.

Of the Extent and Places of the Unstratified Rocks.

Though the unstratified rocks form but a small part of those which are visible to us, there is reason to imagine that, at depths to which we have not penetrated, they occupy a much greater extent than would be suspected on a superficial view. Conjectures have been offered respecting the proportion of space which granite fills at the surface, but no measurement has been given, nor has anything been done towards answering this question as it relates to the later unstratified substances. In fact, these vary so much in different countries, that if any worthy object were to

be gained by that knowledge, it could still only be done after the whole surface of the globe should have been surveyed and reduced to a geological map. It would be now easy to solve this problem, as far as it relates to Britain, as well as to some other European districts; but a general expression of the extreme disproportion between the stratified and unstratified rocks is all that can here be necessary. In Britain, it may be stated that the whole of the latter, comprising alike trap and granite, do not cover a thousandth part of the superficies of the island. If we examine the flat tracts of Russia and Poland, and of other similar countries, thousands of square miles will be found not containing the least vestige of these rocks.

Although the unstratified rocks do not necessarily occupy the summits of a country, they are found principally in mountainous, or at least in hilly regions. In the tracts of an uniform low level, to which I have just alluded, they very rarely exist at all. Thus a connexion is traced between the existence of these rocks, and the disposition of the surface, a connexion, it is true, not very rigid, but sufficient, when combined with many other circumstances relating to them, to be of some value in our reasonings respecting their origin and consequences.

It is a familiar observation, that granite forms the highest peaks and ridges of the most elevated mountains of the globe. The remark is, however, more common than true; it is certain that it constitutes many of these, but there are numerous mountains in the first class of elevations that are formed of stratified rocks to the summits. Even where granite exists, it often forms only a portion of the highest points; the sides, even at very great elevations, and many of the ridges and peaks, being still constructed out of the superincumbent strata. In a certain limited sense, it may also be said, that trap forms the highest summits in mountains and hilly regions, a remark very conspicuously true in some parts of the enormous ridges of South America. But granite is also found, even on the level of the sea, in the same districts, as are the different members of the trap family. The theory which will be hereafter given respecting the origin of

these rocks, will be found to agree with the mode of disposition.

When the superficial disposition of the strata is examined, it is found that not only a series of them, but a single bed, often occupies a very considerable and continuous extent of surface; and when, from the occasional superposition of other beds, or the absence of portions, from the effects of waste, they cannot be absolutely traced, still their former extent, or even their present, is easily inferred by the well-known rules of geology; namely, the comparison of characters, relative position, and angles of inclination. But the unstratified rocks are rarely found occupying extensive continuous spaces on the surface, being, on the contrary, interrupted by the intervention of portions of strata, or else appearing in distinct, and often in small eminences, scattered through a district, of which the fundamental part is stratified. The minuteness of these is often, indeed, very remarkable, since in Scotland, where both these classes of rocks abound, it is not unusual to find a portion of granite, of a very few yards in diameter, separated by many miles from any other of the same rock, while the same is frequently observed with respect to trap.

The cause in these two cases is, however, different in its nature. By examining sections, and tracing the condition of the insulated masses of granite, and of the conterminous strata, it is discovered that this rock exists in a continuous state beneath, and the apparent insulation is the result of the absence of the superincumbent strata at those particular points. In the case of trap, on the contrary, where there are not reasons for supposing that the deposits have been partial, it is often easy to discover that their insulation arises from their own waste, the thinner or feebler connecting portions having been destroyed while the others have remained.

It is easily inferred from the above remarks, that, of the unstratified rocks, one set is inferior to all the strata, and another superior to them. But with respect to trap, at least, this is not exclusively true; whether it be the case with granite, is a question that belongs to the specific history of that rock, and

which I must defer to another occasion. The rocks of the former division are found beneath the strata, as well as above them, in masses so large and so irregular, that they cannot be ranked among veins, otherwise than as every finite mass included among stratified rocks may claim to be considered as such.

The forms of all the unstratified rocks are irregular, and, in the majority of instances, totally dissimilar from those of strata. It is owing to this absence of the stratified form and disposition, that we are unable to infer the continuity of the rocks under review, when that is not actually visible. It may be supposed, it is true, that if granite exists beneath all the strata, so as to form their immediate foundation, every mass is really continuous, however deep that connexion may be situated. But, with respect to trap, no such inference can be made. Separate portions may originally have been separate deposits: as similarity of character is insufficient to establish their original identity, and as they do not admit of being compared by positions or inclinations; tests which, from their origin and forms, are inapplicable to them.

Of the Antiquity of the Unstratified Rocks.

The relative antiquity of the members in any continuous series of strata, is readily known from their order of succession. In these, geology even teaches us to distinguish certain revolutions, marking very different and distinct periods or portions of time during which they were formed. But we have no similar criterion by which to judge of the relative antiquity of unstratified rocks that occur together; while, in our judgment respecting the relative antiquity of any of them to the stratified, we must be guided by rules that can only determine that question within certain limits.

With regard to granite, it would be imagined, on a superficial view, as it actually has been maintained, that because it is the lowest, it is therefore the most ancient rock. This is an error arising, partly, from unduly extending the law of succession as it relates to the stratified rocks, and partly from overlooking the

obvious phenomena which attend that substance. It is unnecessary to add the moral reasons, arising from habit, authority, hypothesis, or unwillingness to acknowledge error. The entire arguments must be reserved for a specific history of granite; but I may here remark, that it is chiefly by the passage of veins from a mass of this rock through the incumbent strata, that its relative posteriority to these is indicated. It would be attempting to demonstrate a self-evident proposition, to say that, of two co-existing rocks, of which the one traverses the other, the traversing body is the latest in origin. That such a vein is a prolongation from a mass of granite, renders the posteriority of the latter equally an axiom.

Judging by this criterion, a given mass of granite is posterior in origin to every stratum in a series which is traversed by its veins, or, what is equivalent, is disturbed by its contiguity. Mere contiguity, without disturbance, will not prove it; as strata might be deposited on granite, and as it can be shewn that they have actually been so deposited. But although this test defines the highest limit of relative antiquity, it leaves the lowest indefinite; since, in a deep series of strata, the superior, or distant portions may have been but slightly disturbed, or have entirely escaped disturbance by a granite which has not emitted its veins far beyond its immediate boundary. However certain, therefore, it may be that any mass of granite is posterior to the gneiss, the micaceous schist, or the argillaceous schists which it traverses, or into which it intrudes, we are unable to prove that it is not also posterior to the secondary strata that lie above them, except in those cases already mentioned, where the actual contact is visible. If, therefore, the elevation of the primary strata from the horizontal to the inclined position, is to be attributed to the formation of granite, there is no absolute proof to be drawn, from the non-existence of that rock among the secondary strata, that these also have not been elevated by the same cause. If, in any case of this nature, we cannot prove that a given effect originates in a proposed cause, it is still important to shew that such a connexion is not impossible.

It is evident, now, that the relative position of any two masses of granite to particular strata in the general series, can afford no proofs respecting the relative antiquity of these masses to each other. The veins of the most recent may have been limited, like those of the most ancient, to the lowest stratum in the series. That criterion must be sought, by inquiring whether the veins of one mass of granite penetrate another. And as this is actually the case, it becomes proved that granite has been produced at distinct successive periods. It appears that even three periods can be proved; but it must be evident, that, from numberless causes that cannot here be considered, the difficulty of proving successive productions of it must increase at each stage.

The fact of even two generations of granite is, however, important in that view which considers it as having been the immediate cause or concomitant of the elevation of the strata. It is easy to demonstrate that the strata have actually been elevated at successive periods, in such a manner as to prove that the inferior series was twice moved; and the theory of granite provides the means of producing both the effects as easily as the one.

The question of the relative antiquity of trap rests on somewhat different grounds, and requires a separate explanation. In some place or other, the veins of this substance have been found penetrating every rock from granite upwards; as far at least as the boundaries of those strata that have certainly been deposited under the ocean. Hitherto, they terminate with the chalk stratum. At the same time the masses of that rock lie above this substance, and thus its posteriority to all the strata beneath the tertiary deposits, taking chalk as the uppermost of these, is established. But as trap occupies the surface, and as the upper strata in the general series are not found at every point there, it may of course happen that, instead of chalk, this rock may repose on any other member of the series, as it may even on granite. It must already be apparent, that the mere existence of trap-veins in granite or gneiss cannot prove their antiquity, unless it can be shewn at the same time that they stop suddenly at the point of superposition of some other rock. Neither can the separate

superposition of a mass of the same substance on those, or on any other rocks, prove their affinity to these in point of age. That can be inferred only in cases of continuous stratification among the stratified substances; and if the contrary opinion has been held, it has arisen from a false view of the nature of the trap-rock. Were a continuous mass of trap to be deposited now over the whole of Britain, it would be in contact with every rock in the system. Should certain portions of it disappear through the lapse of ages, there would remain others, independent, and, under occasional circumstances, in contact with granite, with argillaceous schist, or with chalk, as it might happen. Posterity would judge falsely in considering these as distinct deposits, connected, in point of age, with the several rocks on which they immediately repose, and through which each of them would also exclusively send its veins. But this is a picture of the past, as all geology proves; and we are therefore left without a criterion to determine the relative age of any independent mass of trap to the strata, as far as any evidence can be deduced from mere contact. It is important to rectify an error which has led to the establishment of numerical and successive *floetz-trap formations*.

The circumstances, however, under which the trap-rocks have actually originated, and under which they are displayed, render it certain, that there have been distinct deposits at different periods; but it is not less essential, in establishing a fact, to make use of the true evidence, as the false is always in danger of misleading or of proving too much. From the constitution of the porphyries that accompany the older rocks, it is probable that they are of a higher antiquity than the traps found above the secondary strata; but the certainty of two, if not of more successive productions of these rocks, is established by the same fact as it is in granite, namely, by finding veins passing from one mass so as to intersect another. The same is proved by finding masses of trap constituting portions of the conglomerate strata; but as all those minute particulars belong to the specific history of the trap-rocks, they cannot be discussed in this general view.

On the Veins belonging to the Unstratified Rocks.

As the veins of granite and trap are of a distinct origin from the other rock veins, it will be better to consider them in this place, as far at least as it is necessary for the purposes of this general view of the unstratified rocks.

If veins, consisting of granite or trap, have not always been traced to masses of these rocks, that has been done in at least the far greater number of instances, where the attempts have been properly made. That this may not always be possible is easily conceived: the granite mass may be covered by the strata, and the mass of trap may have disappeared. As trap is also, like granite, a subterranean production, it is easy to suppose that veins of it may, like those of granite, be connected with invisible deep-seated masses. This very fact is proved by phenomena occurring in Sky, amply described in my work on the Western Islands. It is further necessary to observe, that the apparent independence of these veins has occurred only where they were of great size; that being the sole condition under which they could exist at great distances from the parent mass. I ought, perhaps, here to add, that I have traced the sources of all those veins in Scotland, which were formerly supposed to be independent. It will not be an illegitimate conclusion, that all such veins are processes from masses of those rocks to which they belong in character and constitution. The sizes of these veins are infinitely various, whether we regard their breadth or longitudinal extent. They have been observed to reach to many yards, and to descend from that to the finest filaments. In their length they also vary; but it must be observed, that the courses of trap veins are far more extensive than those of granite. The larger ones of this nature are known sometimes to extend through spaces of many miles. It has sometimes been imagined that they were unlimited downwards; in which case every vein must be a lamina. That, however, is not the fact in granite, in which every dimension, even of the largest, can sometimes be easily traced. It is equally untrue of trap, as the lateral boundaries of

their planes may be seen and examined throughout, in many parts of the western islands of Scotland. Neither can it be true of either, if we consider their connexions. If, indeed, the depth of vertical trap-veins cannot in some instances be traced, it only proves that the masses in which they originate, and which form their limits, are inaccessible to us. The ramification of veins is a circumstance which requires notice in treating of their dimensions. It is extremely common in granite, but rare in trap; from causes that will be so apparent, in the progress of this discussion, as to require no specific explanation.

The position of veins with respect to the horizon is infinitely varied, as it also is with regard to the intersected strata. Those of granite are generally exceedingly inconstant, while they also pursue tortuous and intricate directions, which defy all attempts to follow them. It is only where they are of large dimensions, and hold long courses, that they appear straight; and in these cases they seem to maintain high angles towards the horizon. But their direction and extent are, in fact, regulated by the magnitude and disposition of the fissures which they occupy, and by the position of the central mass. If, in Cornwall, the prevailing and larger granite-veins hold a common course approaching to the parallel, and if the high angles predominate, it only shows that circumstances in the nature and position of the invaded strata, and in the bulk and form of the invading mass of granite, determined this particular disposition in that place. No general rules can be deduced from any facts of this nature.

With respect to trap-veins, it is a remarkable fact, that they are rarely congregated into one spot in minute ramifications; although I have elsewhere shown that this is the case in Barra. On the contrary, the predominant veins of this substance are generally of considerable, and sometimes of enormous dimensions, while they continue to hold extensive courses without ramification; a fact rarely occurring in the veins of granite. This circumstance implies one, or both, of two things, in which the generation of granite and trap-veins have differed; namely, a different condition of the invaded strata, and a force exerted in

a different manner. Perhaps both of these are correlative propositions. Numerous circumstances, elsewhere scribed, prove a flexible condition once existing in many strata, and most conspicuous in the oldest ones. These are the rocks which granite has especially invaded, and such limited and tortuous fissures are precisely what might be expected from an imperfect rigidity of the strata. The strata invaded by trap, on the contrary, rarely contain indications of flexibility; and thus are accounted for the greater decision and straightness of the fissures into which the liquid rock has flowed. It is interesting to remark how well the several parts of a system hold together, and how readily a true theory is applicable to phenomena which had not entered into that mass of facts on which it was founded.

Most apposite confirmations of this view of the different effect of fissures on flexible and on rigid strata, and on those which yield most easily, compared to those which oppose the greatest resistance, is contained in some of the veins of the western islands, as in Lungæ and elsewhere. One of them is particularly noticed in the account of that island, in the work to which I must here often refer for facts; but while they show the different effects produced on different strata, they serve to illustrate differences in the forms of trap and of granite veins, which are of a very important nature. In one of these, the vein has intersected an alternating series of argillaceous schist and quartz rock, breaking the latter in a decided manner, but bending the former, which consequently bears the same marks of flexures that are seen where granite traverses gneiss. In the others there is a striking difference in the forms of the veins, where the fissure has been transverse to the lamina of the invaded schist, and where it has conformed to these; the vein having the usual parallel form of trap in the former case, and resembling the irregular veins of granite in the latter.

As the veins of trap are most frequently of large dimensions, so they are generally either vertical or inclined at high angles to the horizon. Thus probably they indicate, by both circumstances, the great depths at which the parent masses lie, and the nature

and place of the force which produced the fissures. But that they frequently also occupy under very low angles, will immediately be shown, when the positions of veins with respect to the strata are considered.

Although the term fracture implies that the strata are dis-united in some direction across their laminar arrangement, it does not follow that all veins hold courses thus intersecting them. While they may be broken at any degree of obliquity, they are sometimes also separated according to the planes of their stratification, or according to that of their laminar structure. In the latter case, the veins are rarely large, and are more or less parallel to the laminæ of the invaded rock; in the former, strata, either similar or dissimilar, are sometimes completely separated, even for considerable spaces, so as to permit a large and parallel mass of the intruding rock to occupy the vacuity. The first of these cases is common with granite, the latter with trap; although, even in this last rock, veins are sometimes found intruding into the schistose structure or parallel to the stratification of the conterminous strata. In both cases, an incorrect or prejudiced view of these appearances has given rise to the notion of trap and granite being stratified; with what truth will be judged, when the following obvious remarks by which these are to be distinguished from real strata are described.

Where the courses of granite veins are parallel to the laminæ of the including rock, their true nature is easily distinguished, as they rarely persist, even for a few inches, without some deviation or ramification. In the case of trap, however, as they are often of considerable extent, it requires more care to trace them to their irregularities or terminations. But they present other indications of their real nature, by occasionally sending out branches, or by the effect they have produced on the adjoining strata. The nature of the rock, indeed, as proved from other appearances, ought to be sufficient to satisfy the spectator that they are veins, and not beds; but as it is probable that real strata of trap do actually occur, I must refer the minute parts of this discussion to a special history of the trap rocks.

Respecting the relation, in point of time, between these rock-veins and mineral or metallic veins, it is not probable that any general rules can be laid down. In Cornwall, granite veins, as well as veins of porphyry, traverse the mineral veins; but as mineral veins are also found in granite, even in that district, it is certain that they are of a time intermediate between two formations of that rock. That ordinary trap-veins should generally be posterior to all mineral veins, follows, of course, from the period at which these appear relatively to have been formed.

Many chemical and mechanical changes occur in the including strata at the places where they are traversed by veins, whether these be of trap or granite. But these are both numerous and minute; while such of them, as are not necessary in the present general view, must be reserved to a more proper place, should an opportunity offer.

But, in this place, where the question of veins has been treated generally, as far as that was possible, it is important to observe that the effects are, in both instances, not only similar, but as exactly identical as could be expected when the various differing circumstances attending them are considered.

On the Origin of the Unstratified Rocks.

The reasons for believing that all the unstratified rocks are alike of igneous origin, or that they are substances crystallized from a fluid of fusion, belong, in detail, to a different essay from the present one. The following remarks on the mode in which these rocks assumed their present forms, and on the probable causes and effects of their fluid state, proceed on this supposition. In a science in which there is so universal a re-action of all the parts, and where every set of facts is necessary to illustrate every other, all cannot have that first place in consideration to which so many have a claim.

As it is apparent that granite has been in a state of fluidity beneath the strata, and that, during this state, these have been elevated in an irregular manner, it is easy to account for the irregularity of its general surface, or for the partial way in

which it is found distributed on the earth's superficies. The consequence of the unequal elevation of the strata, was to produce those interior inequalities that have been filled by the yielding mass which was the immediate cause of that fracture, and the concomitant of the force exerted. The production of the veins is another obvious consequence of the fractures or discontinuities formed by the displacement of the strata. It must be remembered here, however, that the actual appearance of granite at the surface of the earth is, in most cases, the consequence of another train of effects, consisting in the waste of those parts of the strata by which it was once covered; a waste, of which the whole globe produces the most unquestionable evidence. From the progressive state of that waste, it follows, that the apparent quantity of granite in the earth must be constantly increasing, although itself subject to decay; and if it really be the basis of all the stratified rocks, it is possible to conceive, that the higher parts of the earth might, at some future day, contain granite only; since the perpetual abstraction of unity from two quantities, one of which may be regarded as nearly infinite, must finally terminate in the ratio of one to nothing.

Since now the other division of the unstratified rocks is found above the strata rather than below, it is necessary to inquire respecting its source or origin. That it was produced in a fluid state, and consolidated from that condition, rests on precisely the same ground as the case of granite. The nature of the different substances is similar, often identical; the effects are the same on the including strata, and the disposition of the veins is strictly analogous, varying only according to circumstances that have already been stated. Nor is trap exclusively superficial; since it is actually found beneath the strata in considerable masses, or else in such a relative position to them vertically, as to resemble granite in this respect; while, it will further be shown hereafter, that many of the extensive masses of these rocks, now visible at the surface, have actually been once covered by strata that have disappeared in the progress of decay.

It would be an obvious, yet a superficial conclusion, that the

trap-rocks have been deposited, like the accompanying strata, from above. But the moment that their igneous origin is admitted, that opinion falls to the ground; as there is no external source whence they could have been derived. By the argument of dilemma, therefore, we must seek their origin in the same regions that produced granite. That this really is their origin is further proved by the positive arguments derived from the masses that lie beneath or among the strata, by the depth and magnitude of their veins, and by the marks of force which accompany their juxtaposition to the strata. If any further doubt could exist, it would be removed by the phenomena of volcanoes. The substances which these produce are not only strictly analogous in all their essential characters to some of the trap-rocks, but often undistinguishable; while the variations which do appear admit of an easy explanation, from differences easily defined. These rocks, having passed through the strata, flow over them in certain cases; while, in many others, there is reason to suppose, from the effects following the earthquakes that accompany them, that they have intruded among the strata beneath the surface, so as to have produced those well-known permanent elevations of the land found in volcanic countries. That they elevate the superficial strata, is fully proved by the phenomena attending the volcanic coral islands.

It is in the deeper regions of the globe, therefore, that we must seek that of trap, in those where we have found the origin of granite. These substances are essentially of the same nature, but they have been produced at distant periods of time. In accounting for the present superficial position of trap, we are provided with two resources; that of its flowing out in the manner of lava so as to cover the strata, and the final removal of these, so as to leave bare that which was once concealed beneath them. This question is worth pursuing a little further.

If chalk be the uppermost marine stratum in any spot, and if a trap has been erupted under the sea, as many appearances indicate, it must have flowed over the surface of that rock. It may have flowed above many others; as it is by no means certain, nor

even probable, that chalk has been formed everywhere. But respecting this we cannot hope to acquire any satisfactory evidence; as, in the present state of the earth's surface, it is impossible to determine which strata have been removed, and which were never deposited.

On this supposition we may also account for the partial existence of the trap-rocks. It is not necessary to suppose that they have been universal, or even much more general than their present remaining indications now show. Although the materials of trap may have existed beneath, as generally as those of granite have been supposed to do, it does not follow that they should everywhere have reached the surface. The analogy of volcanoes here comes again to our aid: indicating, by facts that cannot be disputed, the possibility, or rather the probability, of this supposition. It is on this ground also, that we may readily admit successive formations of trap-rocks; even if we had not those proofs of it which have already been mentioned: and it may thus even prove, that many of those now known, which are in contact, separately, with different strata in the series, may actually have been of different periods, and not the consequences of a single deposit on rocks of different natures; separated itself into distinct parts, by the effects of waste. It is not a little remarkable, on this view, that two deposits of trap, of which the distant succession is proved by appearances that cannot be mistaken, are found in the same place; a phenomenon exactly similar to the renewal of volcanoes at distant intervals, among the ruins of those long since extinct.

It must thus be apparent, that whatever differences may exist between trap and granite, whether in their relations to the strata, or their mineral characters, they are strikingly analogous in almost every essential general circumstance, and that the former may, in a certain sense, be considered as a recent granite, as the granite of the newer strata. The circumstances respecting these analogies which have not here been noticed, could not have found a place without inconveniently prolonging this paper, and must be referred to a special history of granite and trap. It remains to

account for one difference, on which much stress has been laid, not only by those who deny the igneous origin of every rock, but by those who, unable longer to blind themselves respecting that of trap, reserve all their force in favour of the aqueous origin of granite.

It has been said that granite, in a state of fusion and protrusion, ought to have been erupted so as to flow over the strata, and that, like the trap-rocks, it ought now to be found in that situation, which it is not. This is a proposition apparently very simple; but the simplicity of a proposition ought to be well ascertained before it is advanced as such.

In the phenomena that attend volcanoes, it seems certain that volcanic matter is sometimes moved, or perhaps introduced among the strata, without appearing at the surface. The cause is here indicated by that eruption of rock which takes place at a distant point at the same time. The condition of the surface in many parts of Italy, and the ancient changes of level which it has undergone, seem to bespeak the agency of this cause, if it were not proved in many other cases by those alterations in the form of the surface which have attended earthquakes. The elevation of coral islands proves that additions of solid matter have been made to the strata beneath them, as there is no other mode of solving this phenomenon; while the actual eruption of volcanoes in the neighbouring parts of the ocean, leave no doubt with respect to the nature of the cause.

If, in such cases as this, we conceive the surface to be removed, as it will be at some future period, we should probably find many places where the appearances would resemble those produced by granite; that is to say, masses of fused matter which have not overflowed the strata.

The next step is to apply this reasoning to the trap-rock. If, in any place, the superficial masses of these were to be entirely removed, we should discover the openings whence they had flowed; as we have already probably done, in many cases, by ascertaining the places of veins. Here then we should find, what perhaps we have already truly found, the unerupted trap, with the

strata reclining against it, as in the case of granite. If we conceive that, by a still further process of waste, not only the erupted trap, but the subjacent strata themselves, should have disappeared, we should arrive at the fundamental mass, and find only the slender remains of highly-disturbed strata, covering this interminable source of the erupted rocks. Trap would then be to us as granite; nor would any proofs of its former eruption and overflowing remain.

Now this is not a mere postulate. In the district of Morven, a mountainous mass of trap, attaining to twelve or fifteen hundred feet in height, meets a similar mountain of gneiss in a line not far deviating from the perpendicular, its base being lost beneath the sea. Here the gneiss reposes on, or meets the trap, precisely as it would a mass of granite, and is in the same manner disturbed at the junction. This trap mass is indeed connected with a portion that covers secondary strata, and which may be considered as its erupted part. But this is fast wasting away; and the time, however distant, must at length arrive, when the trap of Morven shall present all the geological appearances of granite, and when, should it accidentally have possessed the granitic mineral character of some of the syenitic traps of Sky, it would be supposed an unerupted rock, and a granite.

Similar appearances occur in Sky, where masses of trap, that, as far as we can discover, are interminable downward, pass through the secondary strata, which are consequently found in the same relative position to them that the primary strata sometimes are to granite. One of these is many miles in diameter; and, did neither Sky nor any other district preserve the vestiges of erupted and overflowing trap, it might here also be argued that such a mass could not have been ejected from below.

In these last cases there is one circumstance worthy of notice in this question: it is the difference between the manner in which the trap meets the secondary strata, and that in which granite meets the primary. The junctions in Sky are, in many parts, vertical and very precise; although there are the usual limited disturbances and penetrating veins. This difference arises from a

different condition in the invaded strata, which is confirmed by every other circumstance of difference which attends the passage of veins in both, the appearances at the junctions, and those of the strata themselves. These were noticed before in the case of veins, and they arise from the yielding and flexible nature of the strata in the one case, and their rigidity in the other; facts fully evinced by the contortions which attend the primary stratified rocks. The very quality of the rocks themselves, independently of softness, may have contributed to these differences; as is proved by the instance in Morven, where the lateral junction of the trap with the gneiss is of a very different character from those which take place between it and the secondary strata of Sky. So far, therefore, from its being proved that the mode of junction between granite and the incumbent strata is not the consequence of eruption, because it does not resemble the junction of trap in similar cases, these differences are precisely of a nature to confirm that opinion. If, in mechanics, unequal bodies are projected with a common velocity, we infer that two forces have acted; if the velocities are different, we then inquire whether one force may not have acted on both. It is not probable that geology will ever be ranked among the accurate sciences; but we may here infer, from a balance of phenomena, that which we cannot demonstrate.

There is yet a case of unerupted trap, of which Sky produces a distinct example, and which is relevant to the question under consideration. It has also been observed elsewhere. In this instance, a mass of trap, untruncated downwards, and of a conoidal form, is seen, in a fair and deep section, covered by the secondary strata, which are so bent over it as to be accommodated to its form. Here is a case precisely analogous to that of an unerupted granite; and it is obvious, that when, in the progress of waste, its summit shall reach the surface, it will present an example of unerupted trap, with the strata conforming to it on each side, just as the primary are so often found to do in mountains of granite.

We may now inquire how these facts apply to the state in

which granite is supposed, by the postulate, to be invariably found, unerupted, and not lying on the strata to which it is contiguous.

The last case of trap quoted, is here applicable and perhaps more completely than would at first be imagined. If it was not necessary that a protruding and protruded mass of trap should in every instance make its way through the superincumbent strata, neither has it been in the case of granite. Even the rigid sandstones of Sky have yielded to the pressures of the trap; and much more must it be believed that this has happened in the case of granite, when the softness and flexibility of the primary strata in its vicinity are considered. I need scarcely add that the volcanic elevations of strata, without eruption, present the same analogies.

There are moreover circumstances in the constitution of granite, which prove that it has been under the influence of heat for a longer period than the trap-rocks. This is its more perfect and distinct crystallization; a difference which we are enabled to produce, even in our laboratories, by prolonging the fluid state of fused traps, and which is a common occurrence in volcanic rocks, under similar differences in the rapidity of cooling. I have, in a former paper in this Journal, attempted to show that gneiss itself is the evanescent limit between granite and the other stratified rocks. Thus it is at least possible, perhaps even probable, that in many cases where granite has actually been erupted, the rocks which it covered have, by long exposure to its action, and by being thus involved in it, been converted into its substance. This supposition is not in want of support from analogies furnished by the trap-rocks, as an instance occurs in Cantyre as well as on the continent of Europe, where the gradual conversion of a schistose rock into porphyry, under similar circumstances, is proved in a most unquestionable manner.

In the next place, we may apply to granite, as we now find it, the cases of apparently unerupted trap already examined. The immense deposits of materials which now form the alluvial tracts of the globe, the enormous masses of secondary strata that have

been produced by ancient materials of the same nature, all prove the magnitude of the destruction which mountains have formerly experienced, and which they are now daily undergoing. Let imagination replace the plains of Hindostan on the Himalaya, or rebuild the mountains which furnished the secondary strata of England, and it needs not be asked what is the extent of ruin, modern and ancient. In this ruin, the highest rocks participate most largely, so largely that we can scarcely hope to find one portion of that surface which was once most elevated above the waters. If, in the progress of such extensive destruction, thus probably acting on the primary rocks at two distant periods, every vestige of overflowing granite has disappeared, it is assuredly an event not calculated to excite surprise.

That granite has in reality furnished a very large part of the materials of the recent strata, is proved by their constitution. Quartz, felspar, mica, and hornblende, are the chief materials of the sandstones, shales, and clays; nay, the very fragments of that rock are found everywhere. Even in our recent alluvial soils they abound; and it is a question worth considering, whether the granite boulders, of which the immediate origin has so often vainly been traced, are not rather the portions of decomposed conglomerate strata, or the durable remains of the alluvial soils on which they now repose.

In an argument thus made up of probabilities, and resting on analogies, it is legitimate to muster all the force that can be adduced. We have yet to learn whether, if the secondary strata were laid bare, the granite might not often be found beneath them and incumbent on the primary; protected from, at least, the second period of destruction, by their covering. It may happen, for example, for anything that we can prove to the contrary, that this very case may be present in Sutherland, where the secondary strata actually repose on granite. The observations on the junctions of granite and the primary strata are not easy to make, nor are the exposures often very perfect; so that this is one of the cases where the difficulty of proving a negative leaves conjecture alike open to all. Why may it not be added that there is not

only an absolute want of observation on this subject, but that the universality of a prejudice in favour of the aqueous origin of granite, renders some observers as unwilling or as unable to discover the truth, to explain appearances in an obvious manner, as others are, from want of experience or other causes, incapable.

This argument has hitherto proceeded on the grounds of analogy and of inferences from causes and effects; but it is time to ask whether the postulate is really true, when it assumes that granite has not overflowed the strata. We have the authority of Von Buch, a testimony never yet questioned, that granite lies above conchiferous limestone in Norway. No evidence can be less liable to suspicion than that of a person who originally denied the truth of the theory of which his own testimony was even then adduced as a proof. I have also shewn in another work, that in Sky, a granite, defining it by its mineralogical character, is an integrant part of a mass of unstratified rocks which lies on a similar limestone. If I have chosen to consider this rock as a member of the trap family, it is merely because I have thought fit to adopt the difference of age as a criterion between granite and trap, for reasons that need not here be stated. The fact, so far as this argument is concerned, remains the same; but it is plain that it will be necessary to abandon this distinction between trap and granite, whenever it shall be disputed whether the latter has ever overflowed the strata. If, in this particular instance, I had not detected the superposition of the mass, it would, of course, have been ranked with granite.

We are thus at length brought to consider the further evidences that may be produced to prove the common seat of granite and trap, and the identity of the circumstances in which they have originated. These are deducible from similarity; in certain cases, from absolute identity of character; but I must premise, that to limit the term granite to the sole compound of quartz, mica, and felspar, is merely to abuse a mineralogical term, for the purpose of evading a geological inference. This is an expedient which, however often used, is inconsistent with the rules of sound logic. In a geological sense, every rock must be

considered a granite, which, whatever its composition may be, forms a portion of a common mass of that rock in its most acknowledged character. In Aberdeenshire, as I formerly showed in this journal, the leading varieties of granite are of that character which agrees with the most rigid mineralogical definition, and the superposition of gneiss over a very extensive tract of that rock can be traced with the greatest facility. But, in many places, a variety of this granite occurs, which is composed of felspar and hornblende only, passing into the ordinary kind by means of the usual fourfold compound of hornblende, quartz, mica, and felspar, and the threefold one composed of hornblende, quartz, and felspar; to both of which the term syenite has sometimes been applied. The continuity and gradation of all these, and their inferiority to the primary strata, can be traced without the slightest difficulty. In examining more minutely the duplicate compound just mentioned, it is observed in many places to assume a fine grain, and thus at length it becomes undistinguishable from the greenstones of the trap family. But the identity does not cease even here; since, in many places, it passes, in the same uninterrupted manner, into a basalt, and, at length, into a soft claystone, with a schistose tendency on exposure, in no respect differing from those of the trap islands of the western coast.

In this case, all these rocks form a common graduating mass, and are therefore of the same date; but, in Shetland, there occurs another instance which presents, with similar features, an interesting variety of origin. In that region there is an extensive mass of red granite of an ordinary structure, accompanied by a dark one composed of the quadruple compound, hornblende, mica, felspar, and quartz. In the latter, the same gradation into basalt may be traced, in a manner equally perfect; and that this mass is granite, is evinced by its inferiority to gneiss, under all the usual well-known appearances. But it is also of a prior date to the red granite, as the latter everywhere penetrates it by its veins, just as it does the approximate strata. Hence its antiquity is no less unquestionable than its character. Thus it is

proved that granite, or at least a rock originating in the same causes, may possess the characters of some of the most common varieties of the most recent traps. It remains to reverse the proposition, and to adduce instances of the granite character among these recent unstratified rocks.

If the rock of Arran be considered a granite, which it is esteemed to be, the same latitude must be extended to that of St. Kilda. Mica is often absent from both; and both contain, in certain parts, cavities, in which felspar and brown quartz are crystallized. But the rock of St. Kilda is connected with common greenstone and augite rock; substances esteemed to appertain to the trap family. If these, instead of being admitted among the traps, are referred to granite, St. Kilda will merely offer an instance to add to the former part of this analogy.

In Sky, a quadruple compound of hornblende, felspar, mica, and quartz, passes into a triple one, in which mica is absent, and at length, by a variety of gradation, into a compound of felspar and quartz, felspar and hornblende, claystone, porphyry, and greenstone. The whole mass is continuous; and, in some place or other, every member of it is found lying above the secondary strata. Here there is a mass of trap, containing a granite, undistinguishable, not only from many of the varieties which occur in Arran, but from those which are so often found beneath the primary strata.

There is little doubt that whenever adequate observers shall choose to follow the course here pointed out, and to pursue, unbiassed, that chain of observation which is here indicated, more instances of the same nature will be brought to light, both among the traps and granites. It is especially necessary, however, to guard against being misled by mineralogical terms. It is not in the minute arrangements of a cabinet of specimens that the great features of nature are to be studied; nor can geology ever rise to the rank of a science, if it is to be cultivated by grovelling among fragments, and substituting words for knowledge.

I may now indeed add, that I have been satisfied by the examination of collections and their collectors, that the same pheno-

mena have occurred to many observers. The case of Predazzo is one where granites of all characters form a mass with augitic greenstone; the whole being later than the red marl and the oölite limestone; and from the observations of Von Buch, Boué, Brogniart, and others, in Norway and elsewhere, it is evident that what I had so long ago pointed out, required only to be more generally known to be found a very common occurrence.

In the meantime I am unable to perceive that anything is wanting to prove the identity of origin in trap and granite. It is likely, at least, that geology will not often furnish us with evidence of a more decided nature. Nor is it an indispensable requisite to this argument, to produce numerous examples; since there are innumerable cases in science, among which this seems one, where one or two facts are as decisive as a hundred.

It is unnecessary to add feeble arguments to strong ones; but the prevailing mineral characters throughout the whole range of the unstratified rocks are of a nature to confirm that which is here proved. Yet it will not be superfluous to allude to the probable causes by which the characters of these rocks have been modified; and which have had an effect so generally steady in distinguishing between those of the families of granite and trap. It is very probable that many of these differences arise from a constitution radically different, from the proportions of the several earths entering into them. In extreme cases, as between claystone and granite, they certainly depend on that cause. To put a hypothetical case: If it be conceived that granite was produced from the fusion of an argillaceous sandstone, and basalt from that of an argillaceous schist, the consequent difference between these two substances is easily accounted for. It is impossible at present, for want of space, to support this case by facts, and it belongs properly to the specific history of the rocks in question; but even where the composition is similar, experiment has taught us that the same mixture will, under different degrees of heat, and with a different management of it, produce different artificial rocks. Slow cooling generates a highly crystalline arrangement; quick, an obscure one. The

reader may be safely allowed to make use of this analogy in his own way; as the writer himself could do little more than apply it hypothetically to the explanation of the differences between trap and granite. Only let it be recollected, that there are many differences in the condition of the earliest and latest erupted materials, and in that of the strata among which they have intruded, which will, with a little reflection, go far towards explaining the differences in question, if the causes and effects cannot be rigidly approximated.

I will not now examine the reasons why the fluidity of granite and of trap must have been a fluidity of fusion, although, as a question common to all the unstratified rocks, it in some measure claims a place here. I am fearful of prolonging this essay too far; and if an opportunity should hereafter offer, it will be found in the detailed histories of granite and trap.

Thus far this discussion has proceeded without involving considerations of a merely hypothetical nature; but it cannot be dismissed without noticing the causes which geological theorists have assigned for the fusion and protrusion of the unstratified rocks. To the expansive powers of a heat, or fire, situated deep beneath the surface, is attributed the enlargement or protrusion of the fluid which it has produced. The arguments shall here be stated in as brief a form as possible, as there is little to be gained by dwelling on illustrations of this nature.

It is argued that heat does operate some of the effects in question, and that it is capable of producing them all; that no other agent is known equal to fulfilling all the conditions, and that the effects are analogous to those which are witnessed in the actions of volcanoes, whether as they relate to fusion, expansion, or elevation of the superincumbent strata. It is next argued that the existence and phenomena of hot springs, and the antiquity, renewal, and existence of volcanoes, prove that the earth is the repository of a deep-seated and permanent source of heat. The questions that regard the real nature of this heat, whether it be permanent, or but occasionally excited, are of too extensive a nature to be introduced into this comparative sketch, of which the object is distinct in character.

Of Veins of Quartz and Carbonate of Lime.

It remains yet to consider those rock veins which belong neither to the families of trap nor granite. The most abundant are those which consist of quartz and of carbonate of lime, and they will be found more analogous to mineral veins than to those which have preceded. They even throw a sort of light on some circumstances, at least, in the formation of those veins; although the greater part of that subject is still involved in impenetrable obscurity.

Veins of quartz are found in granite, gneiss, micaceous schist, and indeed in every member of the primary rocks; nor are they even excluded from the secondary, although in these they are more rare. They present many varieties of mineral character, since they are the repositories of most of the specimens of this substance which are to be found in mineral collections. An enumeration of these forms no part of the present subject. In their dimensions they vary exceedingly; the breadth reaching from many yards to the thickness not exceeding that of paper, and the length, or lateral extent, varying in the same manner. As, however, they have no connexions with masses of similar rocks, their terminations in every direction can be traced, provided access be obtained to these. They are subject to ramification, and, as is elsewhere remarked, they sometimes follow the contortions of the strata in which they are contained.

The same description will serve for veins of calcareous carbonate; but it must be remarked, that the rocks in which these occur are much more limited. They are found in limestone, serpentine, argillaceous schist, shale, some sandstones, and trap; but are extremely rare in micaceous schist, or in the other rocks of the primary division: in granite and gneiss, they have never, I believe, been observed.

The origin of these two kinds of veins has been a matter of dispute among different theorists. They have been called contemporaneous by one party, and supposed to be produced by the same unaccountable crystallization that formed the including rocks. It

will be time enough to examine the validity of this theory when nature shall establish new laws of crystallization, and when the past and the future are alike present. Those who have favoured the theory of an igneous origin or consolidation, have imagined that quartz or carbonate of lime were secreted, in a state of fusion, into fissures formed by the shrinking of the indurated strata. Nothing can be much more gratuitous than this supposition, while, even if admitted, it would not explain the formation of these veins in granite and trap. There appears little difficulty in tracing them to a watery and gradual infiltration of the two minerals in question, whatever objections may be urged against the production of extensive veins of quartz in this manner. The process may, in fact, be traced to a sufficient extent to allow us to infer the possibility of all that is here asked.

The infiltration of quartz and of carbonate of lime through rocks is proved by the formation of chalcedonies, quartz, crystals, and calcareous spar, in the inflated cavities of trap-rocks, and in these every stage of the progress can be traced. It is proved, in the case of calcareous spar, by the very common formation of ordinary calcareous stalactites. It is proved that quartz crystallizes from water in these very veins, because that variety of rock-crystal which is formed, like nitre, not by the process of gradual increment on one central nucleus, but by a process commencing in various distant parts at once, often contains water. Lastly, I have traced the actual formation of these veins in primary limestone in Glen Tilt, where the narrower parts of the fissures were filled by a consolidated spar, and the sides of the remainder were covered by an incipient crystallization, the vacancy containing a solution of carbonate of lime. The end of the process can, in such a case, be easily predicted, and there is no difficulty in applying the same explanation to every analogous instance. I ought to add, in terminating this subject, that the same vein sometimes contains both quartz and calcareous carbonate, a case exactly similar to that of the silico-calcareous nodules found in trap.

Veins of felspar and of compact felspar are not uncommon in those rocks which contain veins of granite and porphyry. It

would be superfluous to describe them particularly, as they differ in no respect in their features from those of granite and trap. It will be found that some of the veins of common felspar are varieties of granite veins, as is frequently indicated by occasional crystals of imbedded quartz, and that the others are the bases of porphyries, in which the usual imbedded crystals are rare, or altogether wanting.

It is barely necessary here to mention the existence of pitch-stone veins, since, while it seems to be only the form in which that rock occurs, I have formerly examined that subject in this Journal.

The last species of veins requiring notice are those conglomerate veins which cannot be ranked with the mineral veins, but which were slenderly noticed in a former paper on that subject. They are probably very rare, as I have met with only three instances of them. In these the materials consist of various rounded stones, with clay and sand, cemented in the usual manner of conglomerate rocks, and they have evidently been formed by the casual falling of the loose materials into open fissures.

ART. II.—*On the Hair Hygrometer*. By J. Prinsep, Esq.

[Communicated by P. M. Roget, M.D., F.R.S., &c.]

MR. DANIELL, in his late papers on Meteorology, published in the *Quarterly Journal of Science*, has so shaken the confidence of philosophers in the power and accuracy of Saussure's and De Luc's hygrometers, to the advancement of his own ingenious invention, which ascertains the quantity of moisture in the air by the temperature at which the latter begins to deposit dew, that it may appear daring to stand up in defence of the antiquated instrument.

2. A series of experiments, however, with such an instrument, which, for want of a better, I lately constructed at Bunarus, has convinced me that quite sufficient accuracy may be attained by it, with certainly less trouble of observation; nothing more being

necessary than a reference from the degree pointed out by the index to a table of aqueous tensions calculated for the instrument; whereas, in the dew-point hygrometer, the operation is much more complex, and an experienced eye can still hardly estimate the point when the first dew appears, nearer than a degree of Fahrenheit's thermometer.

M. Biot, in his excellent *Traité de Physique*, has described the most perfect form of the hair hygrometer, and has given a formula and table for the value of its degrees, founded on the experiments of M. Gay Lussac and M. Dulong. Where an experimentalist of no comparative fame ventures to advance results at variance, though in a small degree, with such high authorities, it seems incumbent upon him to enter into such details as may not only show how fairly his results were deduced, but how capable were his means for insuring accuracy.

I must, therefore, hope to be pardoned a little prolixity, as well as some preliminary explanation, for the benefit of those who are not quite conversant with hygrometric subjects.

3. I have represented, in fig. 1, a perspective view of the hygrometer: it differs little from the form used by Saussure, except in carrying * two hairs instead of one, each with a separate cylinder and index, so that it embraces the advantage of two instruments on one frame. There is a sliding clamp above, to regulate the length of the hairs; and there is also a rack and pinion-adjustment to the divided quadrant, to correct the zero point of the scale, without touching the more delicate parts of the instrument.

4. The arc is divided into 100° ; 0 being the point where the indices become stationary in extreme dryness, and 100 the same

* I must not claim any originality in the use of two hairs, for I find in a Journal of Science just received from England, that M. Babinet has recently employed three hairs on one frame. In mine, one hair (No. 1) was in its natural state,—the other had been washed in alkaline water; the latter became more sensible by one-sixth in consequence, but after a month or two it did not reach the zero point in extreme dryness by 3° , while the natural hair returned precisely to the original point.

point in extreme moisture. These two fixed points are necessarily found (like the freezing and boiling points on the thermometer) before the scale can be graduated. Supposing the hygrometer to be thus far completed, it only remains to be ascertained,

First. Whether the zero and 100th degree correspond to the state of minimum and maximum moisture of the atmosphere at all temperatures?

Second. Whether the intermediate degrees are proportionate to intermediate states of moisture? or, in technical language, what is the ratio between the hygrometric degrees and the actual tensions of aqueous vapour?

5. The point of extreme moisture is obtained by enclosing the hygrometer under a glass receiver, along with a small capsule of pure water, which soon thoroughly saturates the enclosed air with vapour, and sooner, if the medium be rarified by the pneumatic pump. The index then sinks to 100° , provided the temperature of the air and of the water be precisely the same, or rather if the latter be not colder than the air; in which case saturation will not take place. It was well known to Biot, that the absolute heat was of no importance; for its sole effect is so small upon the hair, that the index does not sink sensibly lower than 100° , when the instrument is suspended in the steam of boiling water.

6. A difference, however, of a degree or two between the temperature of the air and that of the liquid is quite sufficient to destroy the equilibrium of tension, and the following experiment will show the necessity of attending particularly to this point. One series of my observations was rendered useless by my neglecting to place a thermometer in the liquid as well as in the air of the receiver.

7. Fig. 2 represents a receiver containing the hygrometer, two thermometers, and a cup of saturated salt brine, which will be seen hereafter to have the power of drying air until the hygrometer stands at $89^{\circ}.5$. The receiver dips into mercury at

the bottom of a cylindric glass jar, into which boiling water is poured, so as to envelope the whole apparatus. The following readings were taken * :—

Thermometer in Water.	Thermometer in Air.	Hygrometer.	REMARKS.
137 °	160 °	74 °	} The salt effloresced on the side of the cup.
139	152	85	
136.7	138	88.7	
136	136	90	Equilibrium of tension.
135	133.2	91.2	} The salt deliquesced again.
130	121.5	91.8	
117	116	91.2	
111	110	90.5	Nearly equilibrium of tension.
92	91.7	90	

8. Having thus premised the caution to be used in equalizing the temperature of the solution, and the atmosphere upon which it is to act, I will proceed to the method of finding other degrees of the scale.

9. The point of extreme dryness is obtained by substituting for the capsule of pure water in Art. 5, one filled with concentrated sulphuric acid, of a specific gravity not less than 1.840, suffering the instrument to remain exposed to its action for at least twenty-four hours. The arc included between the two points thus found is divided mechanically into 100 parts, and their value now remains to be estimated.

10. When the tube of a barometer is moistened with water (well purged of air) and filled with mercury, the level of the latter will stand lower than a dry barometer, by a certain quantity, which is called the aqueous tension, or the length of a column of mercury capable of being supported by the vapour of water at the temperature given. Dalton, Ure, and others, have formed accurate tables of these tensions for every degree of the thermometer.

If any salt or substance is dissolved in the water, having the

* The hygrometer here used was not the standard instrument, but was regulated by it: this accounts for the point of equilibrium being 90° instead of 89° 5.

power of raising its boiling point above 212° , the vapour has its tensive power reduced proportionally throughout the scale. If for instance at 80° , when aqueous vapour supports exactly one inch of mercurial pressure, a mixture of sulphuric acid and water should only support half an inch, its tension would be 50 per cent. of the former, and a cup of such a solution placed in a limited atmosphere would, when in equilibrio, bring the hygrometric moisture of this atmosphere to one-half of what would be present in a state of saturation. All that is necessary, therefore, is to prepare a number of such solutions of different tensive powers, to try them in the barometer, and to leave them under a receiver for some hours with the hygrometer.

11. Instead, however, of using a long barometer-tube, as I believe M. Gay Lussac did, I preferred short tubes, of about seven inches long, which could easily be cleaned, and thoroughly deprived of air. I mounted, as in fig. 3, one fixed, well-boiled, low-pressure barometer, with a glass cistern, and made another similar, but capable of being taken to pieces easily. The scales of both were cut on the tubes, and in the latter one a tall nonius was contrived, floating on the mercury of the cistern. In this the required liquid (previously boiled) was poured, and then mercury filled in with a glass syringe, to the displacement of all the liquid except a minute film. The two barometers were enclosed under the air-pump, and a partial vacuum made; the heights of both instruments were then read off, in different parts of their scales, so as to obviate errors of division or observation. Thus, with pure water, the following results were obtained:—

Thermometer.	Tension by J. P.	Tension by Dalton's Table.
73.5°	0.816 in.	0.809 in.
80	1.005	1.000
82.2	1.092	1.095
91.7	1.427	1.428
68.5	0.686	0.687

Sulphuric acid, of specific gravity 1.840, gave the following measurements:—

Temperature.	Dry Barometer.	Wetted Barometer.	Difference.
77°	Inch.	Inch.	Inch.
	4.92	4.92	0.000
	4.97	4.995	+0.025
	3.01	3.01	0.000
	3.09	3.085	-0.005
	1.79	1.80	+0.01
	1.915	1.93	+0.015
	2.45	2.44	-0.01
	2.945	2.94	-0.005
	2.84	2.825	-0.015
	4.35	4.35	0.00
	4.96	4.955	-0.005
	5.77	5.76	-0.01
	5.79	5.80	+0.01
		Mean tension	0.00

In the same manner a concentrated solution of common salt yielded me these results * :—

Thermometer.	Dry Barometer.	Wet Barometer.	Difference.	Tension deduced.
79.9	Inch.	Inch.	Inch.	In hundredths of aqueous tension.
	3.255	2.49	-0.755	
	2.16	1.41	.75	
	2.50	1.72	.78	
	2.61	1.82	.79	
	2.73	1.95	.78	
	2.91	2.145	.765	
	6.07	5.31	.76	
		Mean . .	.768	76.8
77.7	5.00	4.31	-.69	76.7
	5.22	4.70	.72	
	1.45	0.75	.70	
	1.56	0.86	.70	
	1.74	1.05	.69	
		Mean . .	.70	
81.	3.785	2.985	-.80	76.9
	8.83	3.04	.79	
	3.91	3.115	.795	
	3.96	3.145	.815	
	4.35	3.550	.800	
		Mean . .	-0.800	
				Mean . . 76.8
				Mean . . 76.7
				Mean . . 76.9

* This table shews a gradual increase of tension, with a rise of temperature.

I made

12. It is unnecessary to give the details of my other experiments, which are condensed in the subjoining table: the last column of this table shews the degree at which the hygrometer became stationary with each solution, after repose, in most cases, of twenty-four hours. The two indices kept always within half a degree of the same indication, making allowance for the minor range of the second hair.

Solution.	Specific Gravity at 80°	Tension by Experiment.	Tension by Interpolation.	Hygrometer. Hairs.		
				1	2	Mean.
Sulphuric Acid . . .	1.840	0.00	. . .	0 °	3 0	0.0
Ditto	1.648	. . .	3.5	13.5		13.5
Ditto	1.615	. . .	5.0	19.		19.0
Ditto	1.6067	6.2	. . .	23.5	25.5	23.5
Ditto	1.525	. . .	13.0	34.		34.0
Ditto	1.4841	18.8	. . .	46.5	47.5	46.5
Ditto	1.463	. . .	22.0	49.5		49.5
Ditto	1.4313	31.1	. . .	60.	60.	60.0
Ditto	1.333	. . .	37.6	64.		64.0
Ditto	1.3230	54.6	. . .	76.5	76.	76.5
Ditto	1.312	. . .	57.	80.		80.
Ditto	1.2642	68.8	. . .	85.5	84.7	85.5
Sat. Sol. of Salt . . .	. . .	76.8	. . .	89.5	88.5	89.5
Pure Water	1.000	100.		100	98.7	100.0

13. The best mode of viewing the results of such a table of experiments is, as M. Biot remarks, to delineate them graphically, as I have done in fig. 4, where the degrees of the hygrometer are laid down on a line of abscisses, to which their respective tensions, as found above, are made ordinates. Through the extremities of these the curved dotted line is drawn: it is out of the nature of such experiments that perfect uniformity should be found in this curve, but its general curvature strikes the eye at once as being of the parabolic form.

I made some rough experiments to obtain the tension of the same solution at high heats, and found at temperature 209° tension = 80.

212 = 81.

214 = 83.

but I place no dependance upon their accuracy. Good tables of the aqueous tensions of different saline solutions are still, I believe, a desideratum in chemical philosophy.

14. The curve deduced by M. Biot from M. Gay Lussac's experiments was an hyperbola, which would nearly coincide with mine in the upper half, but would gradually diverge from it as it approached the zero of extreme dryness. His experiments were made at a temperature of 50° Fahr., while mine were all made between 80° and 90° ;^{*} and I at first thought my curve would have turned out an hyperbola also, differing only in its curvature. By multiplying the ordinates, however, the present curve was established on firm grounds, and it remained merely to seek some simple formula for calculating a table to correspond with it.

15. Upon trial, I found the ordinates to diverge more rapidly than the squares of the abscisses, though less rapidly than the cubes. The nearest expression appeared to be T , (or the ordinate of tension) is as $H^{2.2}$, or by applying logarithms

$$\text{Log. } T = \overline{\text{Log. } H \times 2.2} - 0.400.$$

From this formula I calculated the line in the diagram; it crosses the experimental line at $H=50$, and is farthest separated from it at $H=25$ and $H=75$. This circumstance allows the application of a very simple correction, which makes the calculated curve approximate very closely to the experimental. The corrections are, at

$H = 0.$	Correction = 0.
10	+ 1.0
25	+ 2.5 maximum.
40	+ 1.0
50	0.0
60	- 1.0
75	- 2.5 minimum.
90	- 1.0
100	0.0

* As the tension of aqueous vapour at 90° is four times greater than its tension at 50° , my experiments need have only one-fourth of the accuracy of M. Gay Lussac's, to stand on the same footing with that philosopher's;—a ground, therefore, it is hardly any presumption to maintain.

Or transposing the last 25 units to the right-hand, thus : 75, 80, 90, 100, 10, 20, 25, 30, 40, 50, 60, 70, 75, the correction may be expressed by

$$C = \frac{25 - m}{10}$$

m being the distance of any degree from the 25th, either to the right or the left.

$$\text{Example at } 70^{\circ}, C = \frac{25 - 45}{10} = \frac{-20}{10} = -2^{\circ}.0$$

$$\text{at } 90^{\circ}, C = \frac{25 - 35}{10} = \frac{-10}{10} = -1^{\circ}.0$$

Upon these data the table at the conclusion of this paper has been constructed ; and I will now present a comparison between the calculated and experimental results :—

Hygrometric Degree.	Tension observed.	Tension calculated.	Error.
0.0	0.0	0.0	0.0
13.5	3.5	2.6	-0.9
19.	5.	4.5	-0.5
23.5	6.2	6.4	+0.2
34.	13.	10.9	-2.1
46.5	18.8	18.8	0.
49.5	22.	21.6	-0.4
60.	31.1	31.5	+0.4
64.	37.6	36.	-1.6
76.5	54.6	53.1	-1.5
80.	57.	58.3	+1.3
85.5	68.8	69.2	+0.4
89.5	76.8	77.1	+0.3
100.	100.	100.	0.

16. The accordance is as close as can be expected in such sort of inquiries ; but I could not rest satisfied until I had submitted the tabular tensions thus determined to the further test of comparison with Daniell's method. For this purpose, I ascertained the dew-point on several different occasions, by means of frigorific mixtures, and the subjoined details will shew that I had good reason to be satisfied with the trial :—

Mr. Daniell's Method.						Hair Hygrometer.		
Date of Experiment.	Temperature of Atmosphere, h .	Dew Point, Temperature N .	Dalton's Tension of Aqueous Vapour at h .	Ditto at N .	Ratio of the two, or actual Tension of the Vapour present.	Hygrometric Degree.	Tabular Tension.	Difference.
Sept.	88.3	79.5	1.295	0.985	76.	89.7	77.6	+1.6
ditto.	89.	77.5	1.32	0.91	69.	86.5	71.1	+2.1
	92.	76.	1.44	0.88	61.1	80.	58.3	-2.8
2 Oct.	82.8	76.6	1.10	0.90	81.8	92.5	83.4	+1.6
12 —	84.	62.	1.14	0.56	49.1	73.	48.0	-1.1
2 Nov.	78.5	61.	0.955	0.542	56.7	78.5	56.7	0.0
6 —	72.5	50.	0.770	0.375	48.7	73.	47.7	-1.0
20 Dec.	66.	43.	0.635	0.293	46.1	72.6	47.2	+1.1
21 —	66.	42.	0.635	0.283	44.5	71.	44.7	+0.2

17. I hope I have advanced sufficient to prove that the hair hygrometer, properly graduated, is capable of deciding the quantity of moisture present in the atmosphere, to within two or three per cent. of the truth; beyond which accuracy I should not think the dew-point experiment could be trusted. I mean not at all to disparage the latter instrument, which has the unequivocal advantage of durability, and certainty of indication, while the hair needs to have at least three points in its scale ascertained by comparison or experiment. In durability, however, the human hair seems by no means wanting, if I may believe a notice lately published in one of the scientific journals, that the hair of an ancient mummy was found to be possessed of its full hygrometric properties.

Bunarus Assay Office,
Dec. 22, 1825.

TABLE of AQUEOUS TENSIONS for every degree of the Hair Hygrometer, calculated by the Formula $\text{Log. T} = \text{Log. H} + 2.2 - 0.4$

with the correction $+\frac{25-m}{10}$, m being the number of degrees

more or less than 25.

Hygro- meter.	Tension.	Hygro- meter.	Tension.	Hygro- meter.	Tension.	Hygro- meter.	Tension.
0		0		0		0	
1	0.11	26	7.5	51	22.6	76	52.2
2	0.22	27	7.9	52	23.5	77	53.9
3	0.34	28	8.3	53	24.4	78	55.7
4	0.48	29	8.7	54	25.4	79	57.4
5	0.62	30	9.1	55	26.3	80	59.2
6	0.80	31	9.5	56	27.3	81	61.0
7	0.96	32	9.9	57	28.4	82	62.8
8	1.19	33	10.4	58	29.4	83	64.6
9	1.40	34	10.9	59	30.4	84	66.4
10	1.63	35	11.4	60	31.5	85	68.3
11	1.9	36	12.0	61	32.6	86	70.2
12	2.1	37	12.5	62	33.7	87	72.1
13	2.4	38	13.0	63	34.9	88	74.2
14	2.7	39	13.7	64	36.0	89	76.1
15	3.0	40	14.3	65	37.2	90	78.2
16	3.4	41	14.9	66	38.4	91	80.3
17	3.7	42	15.6	67	39.7	92	82.4
18	4.1	43	16.3	68	41.0	93	84.5
19	4.5	44	17.0	69	42.2	94	86.6
20	4.9	45	17.7	70	43.6	95	88.7
21	5.3	46	18.5	71	44.7	96	90.9
22	5.8	47	19.3	72	46.2	97	93.1
23	6.2	48	20.1	73	47.7	98	95.4
24	6.6	49	20.9	74	49.1	99	97.7
25	7.1	50	21.8	75	50.6	100	100.0

ART. III.—*Observations on Professor Carena's Monograph on the Genus HIRUDO. By James Rawlins Johnson, M.D., F.R.S., F.L.S., &c.*

[Communicated by the Author.]

My attention having been lately directed to a monograph on the genus *HIRUDO*, by Professor Carena, containing some interesting information, on a subject upon which I have myself been more particularly engaged, I trust I shall not be deemed intrusive, in transferring to the pages of the *Quarterly Journal of*

Science the following observations ; and I cannot but regret that the article in question had not fallen under my notice, before the supplemental remarks to my Treatise on the Medicinal Leech had been put to press.

By the frequent publication of monographs, like that we are about to notice,—brought forward, too, by individuals of acknowledged talent,—the science of natural history cannot but be essentially promoted ;—and by recording one's own observations, in place of copying blindly those of others, the readiest means are afforded of detecting error, and of ultimately securing for this science, that distinction to which, from its importance, it is fully and fairly entitled.

Our inquiries cannot, in this department, from the very imperfect knowledge we at present possess, be too concentrated ; the range of nature being much too vast, and the objects it comprises far too extensive, for any individual to compass, or attempt a satisfactory exposition of, the whole. Great and serious injury, it will be conceded, has been done to science, *by attempting too much*. Circumstanced, then, as we are, in respect to our limited information, is it not a more desirable object to study *genera* that bear some relation to, or affinity with, each other, than to pass into an extended region where we have no sure or steady guide to direct us. Errors have, in this way, the greater chance of detection,—a consideration of no trifling importance, for it not unfrequently happens they come before us under the sanction of respectable names, and, under the influence of such authority, pass current as established truths.

Individuals may now, as in former time, labour hard at the impossibility of framing *a complete SYSTEMA NATURÆ*, one likely to prove permanent, or be generally adopted. The supposition, therefore, is not improbable, that some new discovery may, hereafter, create as much alarm among such persons, as it is said Sir Joseph Banks experienced on the discovery of the *platypus*, the latter part of whose life, we are told, was rendered miserable, because he could not determine whether it was a bird or a beast.

The author of the monograph we intend considering, seems to have, at first, principally, if not entirely, occupied his time, in prosecuting some physiological and anatomical inquiries upon one species—the leech medicinally employed at Turin,—with the view of comparing the result of his observations with that of those who preceded him. During his investigation, he discovered this leech not to be indigenous, but imported from France, chiefly from Toulon and Marseilles.

From the circumstance of its never being met with in any part of Piedmont, or in those places where the *H. medicinalis* exists in abundance; from its different appearance, and from its never having been described by Linnæus, or other naturalists, he was induced to consider it a new species, assigning to it the name of *H. Provincialis*. For similar reasons, and what I have myself remarked of this leech, I am led to form the same opinion.

The Professor thus describes this species:—

H. PROVINCIALIS.

Hirudo depressiuscula, viridis: dorso utrinque lineis tribus longitudinalibus, ferrugineis, nigro-maculatis: ventre viridi flavescente, immaculato, striga marginali nigra:—punctis ocularibus decem.

Long. : pollices quatuor.

Habitat in PROVINCIA, prope Massilium, et Telonum.

There appear to be three species employed for medical purposes, the one just described, the *H. Medicinalis*, or what is commonly called the striped leech, and the *H. Verbana*: the latter, from the figure and description given of it, I take to be the *H. Troctina*, noticed under this name, as a new species, in my *Treatise on the Medicinal Leech* *.

The Professor fully confirms the remarks I have made upon this species, by observing, that it has the same structure, and precisely the same *habitudes* as the *H. Medicinalis*.

Owing to the great scarcity of the latter in this country, partly

* *A Treatise on the Medicinal Leech, including its Medical and Natural History, with a description of its Anatomical Structure.* P. 31.

arising from our late improvements in agriculture by the draining of waste lands, we now stand indebted to France for a supply of the *H. Provincialis*. This species is generally, and I may almost say, exclusively, employed here; indeed, to such an extent has this traffic of late been carried, as to have excited a suspicion that a similar dearth will ultimately prevail of this species, and that, if the importation continue, a sufficient number will not be at length obtained even for the use of the French hospitals. The time may probably be not far distant, when the supply from France may altogether cease; hence the necessity must be more than apparent, of directing our views to the best method of furthering the increase of an animal so extensively and beneficially employed. Many valuable and interesting observations are given upon this point, which it is needless here to repeat, in a recent publication *, comprised in the translation of a memoir upon this subject by Dr. Noble, senior physician to the hospital at Versailles.

Besides the three species employed medicinally, Professor Carena has extended his researches to the several species he could procure from the lakes and fresh waters of Piedmont. For the purpose of studying their habitudes, &c., he preserved them some time in a living state. In this way he has been enabled to supply the omissions, and detect or correct the errors of preceding authors, although it appears strange he should pass over in silence the works in which these inaccuracies abound.

The errors, it may be as well to state, hitherto promulgated respecting the leech, and *still met with in works of respectable authority*, are the following:—

1. *That it possesses no organ of vision.*
2. *That it indicates atmospheric changes with such precision, as to constitute it a very useful barometer.*
3. *That its food consists of the larvæ of aquatic insects, worms, &c.*

* "Further Observations on the Medicinal Leech, including a reprint from the Philosophical Transactions, of two memoirs, comprising Observations on the *Hirudo Vulgaris*, or common rivulet leech, and on the *H. Stagnalis*, and *H. Complanata*, now constituting the genus *Glossopora*. Lond. 1825."

4. *That it will destroy its own species.*
5. *That it possesses the faculty of reproducing lost parts.*
6. *That it is exclusively viviparous.*

The above statement, as it respects the medicinal leech, (*H. Medicinalis*.) is, in every particular, incorrect; and with the exceptions three and four, is equally so with regard to the horse-leech, (*H. Sanguisuga*). This being fully proved by what I have already published on this subject, it will be unnecessary to enter, at present, into any further refutation, than that contained in the monograph of Professor Carena, to which I would more immediately confine my attention.

In regard to the organ of vision, the Professor observes, that the ocular points, or eyes as they are generally called, are real organs, and when viewed by the microscope, and in a strong light, appear to form so many oval holes, pierced in the skin, and covered over with a membrane of a deep blue colour. The dark tinge of the head renders it frequently difficult to detect these organs:—but, if the head be slit open, its inner part deprived of its thick, fleshy covering, and thus prepared, affixed to a piece of glass, the eyes may very readily be distinguished, by simply exposing it to the light. This proceeding, however, is only necessary for the three species employed in medicine, and that of the *H. Sanguisuga* *:—in the remaining species, they are evident on the slightest inspection.

* This species, as affirmed by Linnæus and others, has also been used medicinally. In a paper that lately fell under my notice in the *Stockholm Transactions*, they are stated to have been thus employed in the northern parts of Sweden, but only in those instances where the species usually affixed could not be procured. They must be used, we are informed, immediately upon being taken from their native abode, for if only one day enclosed in a bottle, they cannot be made to bite. It is probable the state of confinement in which I had previously kept them, may have been the cause of the inefficacy of all my efforts to induce them to affix themselves, although I can scarcely be brought to think they would evince more eagerness to bite when recently captured. But if the above assertion be correct, occasional advantage may certainly be derived from their use, in the absence or scarcity of other leeches. Professor Carena, however, seems to think they could never be usefully employed; the moment he had ascertained the form of the teeth peculiar to this species, he renounced making any experiments upon others, for every at-

It is not, however, certain, that those black points, observed in greater or less number upon the several species of leeches, are really eyes ; as our author observes, that a leech, deprived of the anterior extremity, in which these organs are situated, performs movements exactly similar to the entire leech ; for this reason, he prefers designating them, for the present, by the name of *points oculaires*, or ocular points. He is of opinion they are only serviceable to the leech in obscure or dark situations. In such situations, be it remarked, the leech naturally fixes its residence, and there these organs undoubtedly perform their due functions ; enabling the leech not only to seek its own individual security, but to procure proper and nutritious food. Muller, excellent naturalist as he was, was unable to detect any eyes in the leech, and M. Thomas and M. Vitet, from the same cause, affirm they have none. Dr. Kurzman, a still more recent writer, is of the same way of thinking. He regards those projecting tubercles, forming a crescent around the head, as simply organs of feeling, founding his opinion of the leech possessing no such organ as that of vision, from its being apparently so little affected by the light. In the description I have given, in my Treatise, of these organs, I omitted noticing the power the leech possesses, of drawing the skin so closely around these tubercular bodies, as effectually to exclude the light. And this, I think, it invariably does, when exposed to a bright sunshine, for the purpose of preserving this faculty unimpaired. To bring forward, therefore, the circumstance of the leech betraying no sensibility to the light, as an argument that this animal is defective in so important an organ as that of vision, is much the same as to say we ourselves have no eyes, because we cannot steadily regard the sun.

tempt upon himself, to induce them to affix, proved fruitless. The teeth, from their rounded form, he remarks, would rather tear the integuments, than cut them, thus producing painful wounds, ending in inflammation and suppuration. But even granting this leech to prove of service, *medicinally considered*, yet its utility would appear to be confined within narrow limits : this animal, according to the report of those who so strongly urge its employment, evincing not the slightest disposition to bite, unless recently withdrawn from its natural habitation ; illustrating the French proverb, "*Le jeu ne vaut pas la chandelle.*"

With respect to the food of the medicinal leech, (*H. Medicinalis*,) which has been stated to consist of earth-worms, &c., I have remarked, as far as my experiments have enabled me to judge, that it never takes anything under a solid form. In this assertion I am supported by Bergmann. Although he has observed that the horse-leech (*H. Sanguisuga*) swallows its food whole, or by piece-meal, as fragments of earth-worms, &c., yet he has never known this to be the case with any other species. Of the ordinary food of the leeches employed medicinally, Professor Carena gives us no direct information;—though he speaks in decided terms of aquatic larvæ, earth-worms, &c., constituting the food of the *H. Sanguisuga*.

The general opinion of the medicinal leech destroying, when urged by hunger, its own species, which rests, I believe, principally upon the authority of Vauquelin, is perfectly groundless. What, according to our author, who never yet knew an instance of their evincing this propensity, may have given birth to the idea, is simply this, that a horse-leech will seize upon and swallow an earth-worm; and whilst engaged in this repast, the other extremity of the worm will be seised upon by another of its own species: at length the mouths of the leeches meet, and here commences the first struggle for mastery; the stronger or more voracious leech, in continuing its operation, swallows the anterior portion of the other leech, exhibiting in fact, the appearance of one leech about to devour another; but if we wait patiently, we shall perceive one of the combatants relinquish its hold and retire, leaving its antagonist in full possession of the worm. This may have certainly suggested the idea, as the Professor seems to think, of medicinal leeches destroying each other; but, with greater reason, it may be stated to have taken its origin from this unsocial propensity *being actually put in practice* by the *H. Sanguisuga*, not only upon its own species, but almost upon any living creature that falls within its reach.

To notice, otherwise than briefly, the several errors connected with the history of the medicinal leech, would be to occupy uselessly the pages of this Journal, and exhaust the patience of its

numerous readers ; we shall therefore bring our remarks upon the leeches medicinally employed to a close, by Professor Carena's statement, relative to the non-restoration of leeches, when divided into two or more portions. This power of re-production, which it has been again and again affirmed the leech possesses, rests, as I have already proved, upon fallacious testimony : to bring forward, therefore, further evidence would seem unnecessary ; indeed, were it not that the detail into which the Professor enters, in describing his experiments upon this subject, possessed some interest, I should but barely have noticed it.

A leech, (*H. Provincialis*,) that had been recently applied, was cut transversely into three parts, the last week of February, 1820. The middle portion was alone preserved, and the water in which it was placed changed every day. The wound at each extremity cicatrised in the course of a few days, leaving an aperture in the middle, being the section of the alimentary canal. "My first intention," observes the Professor, "in thus treating this leech, was that of remarking if it would re-produce the divided portions ; but I discovered nothing answering to that power which the celebrated Spallanzani ascribes to several animals. Instead of this, my surprise was considerably excited, by the tenacity for life this fragment displayed. It was observed frequently to move about in the porcelain dish in which I had placed it, always advancing by that extremity which, in the entire leech, answers to the head. For the purpose of accomplishing its passage from one place to another, it raised, at the same moment, the two extremities, and afterwards lowered them, elevating then the middle portion. In this manner it traversed the water in every direction, on an horizontal plane. It turned sometimes upon its back, sometimes upon its belly, extending itself as if to swim ; but if touched, contracted itself into nearly the shape of a ball. It frequently changed its epidermis, and I attribute its long preservation to the great care I took in assisting it in this operation ; for had it been allowed to remain, it would have been productive of disease, and thus probably have speedily destroyed it. Having preserved this fragment from the end of February until the middle

of July, I transferred it, upon my being unavoidably absent, to my colleague, Professor Rossi, for the purpose of making his own observations, fully convinced it could not fall into better hands. At this present period, 10th Dec. 1820, it is in a living state, and in his possession, being ten months from the time of its division.

The above experiment is so much in point on the non-restoration of lost parts in mutilated leeches, that it would be useless to bring forward others, the result being in every instance the same. Although leeches thus mutilated will live, according to the Professor's account, ten months, and according to Vitet upwards of two years, yet no instance of the restoration of the lost portions is, I believe, on record; and what Dr. Shaw has observed, in reference to the smaller species, the *H. Octoculata*, *H. Stagnalis*, and *H. Complanata*, in which animals he has stated the power of reproduction almost to equal that of the polype, seems also destitute of any foundation. It still would appear singular that the leech should survive so long a period as that above mentioned, after its division. In my recent publication on the leech, this curious circumstance is thus explained. "The general distribution of the nervous system in the leech, this animal possessing no common centre of life, as the brain, but having it nearly equally diffused in every part of the body, sufficiently accounts for the length of time it is enabled to live, after being divided into two or more portions. Other causes may also be adduced, as the general distribution of blood through the respective vessels over the whole body, the leech having no central organ of circulation; the equal distribution, on each side of the body, of the lateral or air vesicles, and the immense or capacious stomach, filling up or constituting the whole of its internal cavity, consequently containing a sufficient quantity of sustenance for the leech even after being divided; this general distribution, I again repeat, of the organs so fitted to preserve life, fully explains the singular circumstance of a leech living in a mutilated state so considerable a period, although deprived of the power of re-producing lost parts, or those which are defective." *Further Observations, &c.*, p. 84.

Independently of the species employed medicinally, and that of

the *H. Sanguisuga*, which, as before remarked, has been occasionally used for a similar purpose, Professor Carena takes notice of the following :—

1. *H. Vulgaris*. 2. *H. Atomaria*. 3. *H. Complánata*. 4. *H. Bioculata*. 5. *H. Cephalota*, and 6. *H. Trioculata*.

The species Three and Four, differing as they do in many important points from the leech, now constitute a new *genus*, under the name (from their possessing a retractile, tubular tongue, the most marked feature in which this difference consists) of *Glossopora*. The species Five and Six are also referrible to the same *genus*. They differ greatly from the leech in one particular, which I am surprised, open as it was to common observation, should have been altogether unnoticed, *that they never swim*, although enabled, like the leech, to pass from place to place with a creeping motion, alternately attaching the head and tail.

Upon the habitudes, &c., of these several species, Three, Four, Five, and Six, I shall not now descant, but simply confine my remarks to the *H. Atomaria*, and *H. Vulgaris*.

The former having been described by Professor Carena as a new species, I shall here give its character.

H. ATOMARIA.

Hirudo atra-nebulosa, punctis, lineolisque transversalibus, pallidis : margine carneo : punctis ocularibus octo.

This leech is about two inches in length. Although it possesses the same number of eyes as the *H. Vulgaris*, viz., eight, yet its difference in appearance, as well as in size, added to the circumstance of its never being met with in several lakes where the latter is very abundant, induced the Professor to regard it as a new species ; and upon this point, I should imagine, he is perfectly correct.

The history the Professor has given us of the *H. Vulgaris* corresponds, in almost every particular, with what I have myself related of this animal, in a paper published in the *Philosophical Transactions*. He very accurately describes the manner in which it brings forth its young, and seems to have been greatly amused

by the care and anxiety it displayed, in regard to the perfect form and preservation of its ovum.

The ovum of this animal seems to have been known to Linnæus before he had ascertained what it really was: he has described it in his *FAUNA SUECICA*, under the name of *Coccus Aquaticus*. At an after period, in his *SYSTEMA NATURÆ*, he notices this production as the egg of the *H. Vulgaris*, in the following words:—" *Ovum patelliforme, ovale, intus compositum, simile cocco.*"

After minutely detailing the several appearances these ova (or to speak more correctly, capsules, each containing from six to twelve ova) assume, from the period of their deposition to that of their ultimately producing young, the Professor relates rather a curious circumstance, which, as I have never witnessed, I shall now briefly mention. That the young, even after quitting this ovum or envelope, frequently re-enter it, remaining there each time a few hours, keeping up a constant passage to and fro, for several days.

The *H. Vulgaris*, at different periods, varies so much in its colour, that, to an inattentive observer, it may readily pass for a new species. Professor Carena informs us, that he has seen them of a lightish brown, a flesh, and even a bright scarlet or vermilion colour; indeed, he presents us with a delineation of this animal, under these several appearances. Strange, however, as it may seem, the smaller animals are found to be not only subject to this change *in a state of nature*, but may be made to assume different tints *dependant upon the nature of the food they take*. This is the case with respect to the fresh-water polype. These creatures, in their native abode, appear under almost every variety of tint, and from the loss and subsequent recovery of their ordinary colour, arise all those shades we meet with in individuals of the same species, and in the same individual at different times.

This variety in regard to colour, induced Trembley, the well-known author of an interesting monograph on these singular animals, to think, that it proceeded not only from the greater or less quantity of food they took, but also from the difference in

colour of the very aliments themselves ; and was, in consequence, led to imagine, that if he gave them insects of a deeper colour than those he had hitherto supplied them with, they would take a colour more marked than what he had been before accustomed to observe.

His first experiment was to render them of a beautiful red ; for this purpose he had recourse to the *P. Lactea*, whose *intranea* are generally filled with a substance of a bright scarlet colour. The polypi, at the time of partaking this food, were nearly white, but, in the short space of one day, acquired a tint similar to the substance they had swallowed, the colour deepening in proportion to the quantity taken. He afterwards fed them with aquatic spiders of a bright red, when they speedily received a deep tint bordering upon that of fire.

Having thus given them a red tint, his next experiment was to make them black. He selected for this end the *P. Nigra*, and the common tadpole : in a short time they became of a similar colour. This change, however, it may be remarked, did not take place, unless the food was well digested.

He now undertook to give these creatures a tinge of green. Unacquainted with any aquatic insect that could impart to him this colour, he employed the green *aphides*, when they shortly assumed a similar tint.

He concludes his observations, by pertinently remarking, that it may be imagined that it is *in appearance only* they take these different colours ; that it is with them, as with a bottle of clear transparent glass, through which we see the liquor it incloses, and which itself appears to be *of the same colour*. To convince one's-self of the contrary, we have little more to do, he says, than examine a polype attentively, when it is deprived of all excrementitious matter, and when it again becomes elongated. We may then see, even with the naked eye, that its stomach contains no liquid, and that its colour is in fact deeply implanted in the very skin. If the supply of further food be withdrawn, these creatures will still be found, at the expiration of a fortnight, to possess a distinguishable shade of black or red, which gradually,

and at length, totally disappears, in about three weeks or a month, the polype becoming nearly white.

My friend Mr. Dalyell, of Edinburgh, who, in 1814, published an extremely-interesting work on the *PLANARIÆ*, brings forward similar instances, in respect to some of the species he describes; remarking "*that the colour of the entire animal is principally regulated by the tinge of the aliment received.*" Other circumstances also operate in producing a like effect,—as removal from their native abode, abstinence, &c. From the *P. Flexilis* constantly fixing its residence in the mud, it acquires a similar tawny hue; but upon being withdrawn, and kept in clear water, it becomes (as happens to other animals similarly situated) almost white. From the nature of the food with which one of this species was supplied, it soon became of a dark reddish brown. After a short abstinence, it partook of a green marine *Nereis*, an animal of as soft consistence as itself, when its body took a greenish hue, which it required three or four days to obliterate.

The discrepancies observable in respect to colour, *even among individuals of the same species*, are attributable, in the words of our author, "*to the quality of the food alone, and its subsequent assimilation with the substance of the Planariæ.*"

This transition of colour in the *H. Vulgaris*, and other leeches, in the *Polypi* and *Planariæ*, and in several animals it were needless now to enumerate, strongly points out the impropriety of founding a specific character upon what is evanescent. Here I must even be my own accuser; for this impropriety, into which I would guard others from falling, may be fairly charged upon myself. In describing what I considered a new species of leech, I gave it the name, from its deep black colour, of *H. Nigra*, although I am now inclined to think it only a variety of the *H. Sanguisuga*, and the total blackness of its body an accidental occurrence.

To give the appellation, therefore, of black, brown, or red, to any species, liable, from a variety of causes, to change its colour, is not only a misnomer, but calculated, from the confusion it creates, to do injury to a science that possesses many and strong claims to our regard.

ART. IV. *Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by William Thomas Brande, F.R.S., Professor of Chemistry in the Royal Institution, &c.*

[Continued from Vol. XXI.]

VIII.

Decomposition of Rocks.

IN the foregoing lectures, occasional allusion has been made to the relative permanence of the rocks that encrust our globe, and to the agents which give rise to their disintegration and decay; subjects which I propose now to pursue more in detail, and especially to examine the cause and apparent ends of those mechanical and chemical processes going on in the great laboratory of nature, tending to a gradual change of the present appearance of the earth's surface, either by the wearing away of its strata, or by the accumulation of materials, which, by their slow consolidation, appear to be forming new ones.

In the animal, and in the vegetable creation, the elementary substances are in a perpetual circle of modification. The seed expands in the ground, the plant thrives, the tree is formed, it produces its flowers, fruit, and seed, it grows old, declines, and decays, being resolved into those substances by the union of which its various forms and products were at one time or other composed. New plants arise from its decayed parts, and these attain their youth and age, and decay in various times, and under different circumstances, but still presenting us with the same unaltered succession. In the animal creation, one generation succeeds another with equally-unerring uniformity; and though in regard to both kingdoms of nature, some great catastrophe, or natural event, seems to have swept away genera and species that once existed, they have apparently been supplied by others preserving the same general characters, though perhaps differing in particu-

lar circumstances of form, habit, and propensities. Upon these changes our attention is the more immoveably riveted, from their immediate and obvious connexion with our own state, existence, and destinies; and we are less awake to those more slow and imperceptible changes to which the inanimate creation also is subject, and to which scientific inquiry and geological history necessarily call our observation.

If we look upon the landscape that surrounds us, we everywhere discern the fingers of that "slow but sure destroyer, Time," busy in modifying the present aspect and appearance of things. The bold and rugged outline of the mountain-chain, full of broken peaks, abrupt precipices, extensive rifts and caverns; the deepening of the valley beneath, covered with a fertile soil brought down by the neighbouring streams, and bound together and enriched by organic remains: the rapid and turbid river expanding into the calm and undisturbed lake, whence it again issues, as it were, in renovated purity, and with new powers of fertilization; are so many monuments of the devastation which a former order of things has suffered, and the records of the changes that are now going on. Let us examine a little into the powers whose aggregate influence is producing these effects, and endeavour to trace the purposes which are thus fulfilled in the general, as well as in the particular economy of nature.

And first, among the great powers of matter active in producing these changes, heat and cold are obviously at all times concerned in exciting dilatations and contractions, and thus keeping up a perpetual but varying motion among the particles upon which they act. Of such changes, the sun is the grand source, to whose varying influence the earth in its diurnal revolutions upon its axis, and in its annual circumvolutions through space, is always exposed. It may seem that the mere influence of change of temperature derived from the solar rays, must be of little efficacy in disintegrating hard and solid substances; but when we reflect upon the very considerable range of natural temperatures, and on the great receptive and emissive powers, in respect to radiant heat, which the surfaces of some rocks present, very con-

siderable transitions from heat to cold, and the reverse may ensue, and acting upon certain textures, may alone lend powerful aid to the general work of decay.

When the influence of heat is conjoined with the varied agencies of water, the powers of destruction will not only be materially increased, but rendered, in many instances, rapid and irresistible. About two-thirds of our globe may be regarded as covered with the waters of the ocean, having, according to La Place, a mean depth of about four miles. Now, in the first place, this immense body of water, kept in continual motion and fluctuation by the attraction of the moon, and by the influence of winds, contributes materially to diminish the solid boundaries which are opposed to its inroads. How legibly are such effects recorded upon the bold and rocky coasts that environ many parts of our island. Their irregular and precipitous outline, the deep gulphs and projecting promontories by which they are indented; the evident relation of these and similar irregularities to the soft or hard structure, and destructibility of the rocks, as well as to the situation of the shore and force of the waves, show how much the present aspect and extent of the shore has been influenced by the action of the sea. The barren and abrupt cliffs which hang over the deep, the strata hollowed and carved out into all kinds of forms and irregularities, sometimes perforated into natural arches, insulated into columns, or divided into small and scattered islands, are similar and not less unequivocal proofs of the gigantic force and gradual inroads of the ocean, and exhibit so many various stages of destruction; and on such shores, the fragments of rock, once detached, become instruments of further destruction, and make (as Mr. Playfair has expressed it) a part of the powerful artillery with which the ocean assails the bulwark of the land. They are impelled against the rocks, from which they break off other fragments, and the whole are then ground against one another; whatever be their hardness, they are reduced to gravel, the smooth and round figure of which is the most certain proof of a detritus which nothing can resist.

Upon flat shores, it is true that an opposite effect appears often

to be carrying on, and that there the land seems sometimes to be gaining upon the ocean. Still, however, we are to recollect that the materials thus depositing in the form of sand, or of various agglutinations of siliceous and calcareous matters, is derived from the neighbouring hills; and thus, that what the land gains in extent, it loses in elevation; and that as far as this source of its increase is concerned, it is actually a monument of its decay, announcing depredations elsewhere, and showing a mere removal of materials.

Let us now turn our attention to those joint agencies of heat and water, which are connected with its evaporation, and with the formation of rivers. Water raised in vapour is rendered portable even upon the wings of the lightest breeze, and is wafted with greater facility than air itself, into the high and mountainous regions of the land. Where these are of sufficient elevation, it is deposited in the form of frozen mists upon their summits, and gradually accumulating, forms those immense deposits of snow, which, by their own weight, slide down into the neighbouring vallies, and thence descend even into the plain, forming glaciers, and thawing into torrents, which, by their union, ultimately constitute the river that returns its waters to the ocean, whence they again circulate in the same round. Or if the country is comparatively low, if there are hills instead of mountains, and soft and yielding strata, in place of the hard and durable materials of the primary mountains, the precipitated water there filters through the various crevices, and finding an exit in some of the neighbouring lowlands, quietly contributes to the springs and fountains which fertilize the adjacent vallies, and which ultimately uniting into brooks and rivulets, gradually augment their forces, and widen into some gently-flowing stream. Thus the magnitude, the power, and the extent of a river will depend upon the circumstances of its origin, and the nature of the country in which it takes its rise; but it in all cases exhibits to us the result of a perpetual distillation from the ocean, the waters of which are transported to the most inland situations, and tend, in their return to the sea, sometimes to harrow up and destroy, but

always to fertilize and adorn part, at least, of the soil and country through which they return to the deep.

It is contended by the Huttonian geologists, and indeed in some places seems obvious, that the valleys in which rivers run are the work of their own waters. Sometimes it is true that we remark some vast chasm in which a river pursues a turbulent and irregular course, and which, from its abrupt and angular lines, as well as from the nature of the adjacent country, and that of the intersected rocks, seems to announce some great and sudden convulsion, by which the channel it now occupies has been formed, and of which the sides or walls are rather rent and torn asunder, than worn by the slow operation of the waters. This is, for instance, the impression which we receive on examining the shattered and rugged crags over which the river Fyers falls, near Lochness, constituting, when replenished by rain, one of the most impressive cascades in Europe, and exhibiting the union of a thousand mountain-streams, struggling for expansion in a narrow and broken passage, exasperated by huge masses of rocks that lie in their way, and ultimately discharging all their violence of waters by a sudden fall into a great rent or chasm, and in an unbroken stream of more than two hundred feet perpendicular. But here, it is not merely the precipitous irregularity of the river that shows its unnatural course—the rocks themselves are of a very singular description. Dr. Garnet compares the base of the rock to a lava; its *appearance* is rather that of fragments of granite and primitive substances united by semifusion, and agglutinated in various degrees by the agency of fire.

Such cases, however, are among the numerous exceptions to which all geological generalizations are liable; and rivers, when examined as to their course and effects, instead of appearing to flow in vallies, formed for their reception by some accidental cause, *seem* often to have cut their own channels, slowly but surely excavating and eroding the land, and engraving that curious assemblage of lines upon the surface of the globe, which the trunks and branches of rivers exhibit. This is Huttonian doctrine; and if we now suppose this process going on through a long succes-

sion of ages, and when the earth's surface was in a very different state from that which it now presents, when perhaps the primitive chains of mountains were covered by other more yielding strata, it is obvious that the destructive effect of water would be much greater in some places than others; that certain strata would, from their softness, perhaps even from their solubility, yield rapidly to its action; and that others would long resist even its impetuous and utmost attacks, and that it might therefore be expected, that a succession of lakes and cataracts did once exist in many rivers, which have been obliterated. To put this subject in a clearer point of view, let us inquire how far the present beds and channels of rivers exhibit traces of such changes, and then compare this former order of things with the phenomena belonging to our present lakes and waterfalls; thus we shall be able, without the assistance of theory, to form an idea of the decay and detrition which rivers occasion, and of the various causes which have contributed to modify their effects, and establish or alter their courses.

In the first place, successive terraces upon the mountains and hills that form the sides of the vallies in which some rivers now flow, as well as the nature of sand and alluvial matter of the valley itself intersected by the present bed of the river, show that the verdant slopes and picturesque declivities, which constitute its present boundary, were once covered by its waters; that they were, in fact, the bottom of lakes, and that the chasm or breach, by which the waters now find their ready exit, did not exist at some former period, but has arisen from some gradual or sudden breach effected in a weak part of the boundaries of the great basin of water that once covered the country. Of the operation of such causes in the production of the parallel roads of Lochaber, we have formerly spoken; and in further proof of the mode of their formation, we may notice the rounded masses and fragments of rock which lie upon the shelves, and which resemble those often seen upon the margin and in the shallow water of Alpine lakes: and also the difference of the soil in each valley above and below its highest line of shelf; all above this natural

division being found to resemble the bare moorish soil which usually covers mountains, whilst large depositions of alluvial clay, sand, pebbles, and gravel, present themselves everywhere below that point, and are especially accumulated at the mouths of the different valleys. (*Ed. Phil. Trans.* IX., 12.) And again, wherever an isolated hill happens to arise from the bottom of the valley to a height above that of the level of any shelf, a line runs round it at a level corresponding to that of the shelf on the mountains of the side of the glen. These circumstances, then, render the evidence of the aqueous origin of these roads or shelves irresistible, and the absence of all marine exuviæ may be considered as showing the absence of the inroads of the ocean. Let us now suppose a deep hollow, between mountains rising with steep acclivities, to have been formed by great mountain-torrents coming perhaps from heights now no longer existing, or formed in any other way, and that such a hollow is partly filled with water, then the agitation of the surface of this water will tend to wear away its banks, but all being quiet beneath, it will not be elsewhere affected. The water will thus occasion the falling down of part of the materials from above, the finest and smallest of which will be washed away and sink to the bottom, while the larger masses will remain, and slowly accumulate upon the brink, forming a shelf, the width of which will vary with the nature of the surrounding rocks. Now we know that all the great lakes of mountainous countries are nearly of this description, and it is evident that the sudden escape of a part of the water of such a lake would produce an appearance resembling that of the parallel roads. Mr. Dick, in the paper in the *Edinburgh Philosophical Transactions* already quoted, has instanced, in illustration of such a catastrophe, the valley of Subiaco, in the Apennines: following the course of the river Tevere upwards from the town of Subiaco, (which is an Italian corruption of *Sublaqueum*, by which name it was once known,) the mountains soon narrow into a very deep glen, which is suddenly blocked up by a rocky wall about one hundred feet high, and three hundred in thickness, and which is rent asunder, as it seems, at

least, having a perpendicular chasm traversing it only twelve or fifteen feet wide, and occupied by the passage of the river. This barrier is perpendicular towards Subiaco, but upon the other side, which looks up the river, it presents a gradual slope. Having got beyond this rock, the river runs in a deep and precipitous valley, and high up on the face of the hills on the south side, are the remains of the baths of Nero, and the mouth of the aqueduct by which Appius Claudius conveyed water to Rome from the lake which originally filled the valley to that height; and on a level with these, extending round the whole of the neighbouring hills, is a parallel road. This lake and aqueduct are mentioned by Tacitus (*Ann.* lib. XI., cap. 13), and what is remarkable, the same historian, adverting to the residence of Nero, mentions the fact of the Emperor having been alarmed by prodigies whilst in that retreat. *When* the lake broke its boundary, either by washing out a vein of some soft material contained in the crevice, or *when* the strata were disturbed by any other cause, is not on record, but it probably took place after the sixth and before the tenth century. The original passage of the river over the boundary, is shown by the crust of travertino tufa, or stalactite, which encrusts the limestone rock, and which exactly resembles that now forming at Tivoli.

But it is not necessary to go back beyond our own times for events analogous to those which are recorded by the parallel roads, and by the vale of Subiaco. Only a few years ago, a lake burst its barriers in the Valais, and the rush of water carried destruction before it as far as Martigny. Similar inundations are adverted to by Saussure, who says, that in some part of the Alps, where the rocks are composed of easily-disintegrating schist, dreadful accidents occur, from accumulations of water in its natural basins and cavities, which often burst the crumbling sides of their reservoirs, and descend with a terrible impetuosity in the state of a liquid mud, mixed with fragments of slate and other rocks. The impulsive force, he says, of this half-liquid, half-solid mass is beyond description. It tears down rocks, eradicates trees, destroys buildings, and overwhelms and desolates

the fields, by excavating deep ravines, and covering the surface with fragmented rocks, gravel, and mud. To compare great things with small, those who remember the bursting of a vat a few years ago in Meux's brewery, may form some notion of the catastrophe derived even from a very small natural accumulation of water breaking suddenly upon the neighbouring territories.

Thus, then, we have considered some of the most important evidence of the destructive powers of water, as rendered apparent by the nature of the beds of rivers, by the excavation of valleys, by the worn and broken aspect of rocky coasts, opposed to the unceasing warfare of the ocean's waves. If we combine these abstract powers, as it were, of water, with those which it derives from changes of temperature, its energies as a destructive agent become more rapid and irresistible. Many substances of a spongy texture absorb it in considerable quantities, and the contractions and expansions which it suffers in their pores tend to their rapid disintegration. This we see in chalk and sandstone, in Bath and Portland stone, and in several other materials, which are thus crumbled down and worn into dust, often with a surprising degree of celerity. Several of the public buildings of London furnish illustrations of this truth; many of the modern structures in Oxford are already more decayed than their ancient neighbours, constructed of less porous stone. In Henry VII.'s Chapel, as it appeared before its recent renovation, all the laboured sculpture and Gothic fretwork was worn and eaten away in the course of three hundred years, and now it has been modernized at great expense, in the very same material. Indeed, in respect to all these substances in nature, we remark in their rounded outline, in the debris that covers their surface, in their eroded and undermined walls where exposed to the action of the waves, and in the turbid waters of the springs and streams that descend from their summits and sides, the extreme facility with which water, aided by changes of temperature, effects their wearing away and destruction. Its powers in this respect are also truly gigantic upon certain rocks, which, from a laminated or flaky structure, admit it into their crevices,

where it freezes and thaws alternately, and thus gradually loosens enormous flakes and masses, which, sliding down their precipitous sides, shatter and fragment the surface, accumulating below in such quantities as to form a hill at the foot of the parent mountain. Such appearances we see on some parts of the coast of Cornwall and Devon, in many parts of the great schistose ridge in North Wales, and more decidedly in the softer shales, as of Derbyshire for instance, where, in the elevation called Mam Torre, masses and fragments are perpetually descending, and have accumulated into a daily-increasing hill.

But with these mechanical agencies of water its chemical powers are frequently combined, and its *solvent* agencies (in a pure state often very considerable) are augmented, as far as *certain* rocks at least are concerned, by the absorption of carbonic acid, derived frequently from the atmosphere, and frequently from subterranean sources. Thus it is that the waters which traverse limestone districts are often so loaded with calcareous carbonate, as to deposit it upon their banks and precipices in the form of *tufa*, or to petrify substances accidentally thrown into the stream or fountain, as we see in the petrifying-well of Matlock; or where these waters trickle through the ceilings, and pervade the walls of those natural caverns, in which limestone strata abound, they stud their sides with an infinity of brilliant crystalline deposits, or form depending stalactites, that hang from the roof in a thousand fantastic forms; or, in other cases, they slowly deposit their once-dissolved contents, so as completely to obliterate entire cavities, which thus become quarries of that beautiful lamellar spar, called *stalagmite*. Of all these peculiarities in the action of water upon limestone strata, Derbyshire furnishes instructive and interesting illustrations; and in the caverns in the calcareous sandstone of Fontainebleau, the yet more remarkable phenomena of the crystallization of the carbonate of lime enveloping sand, so as to alter its texture, but not its form, occurs. Lastly, let us ascend to the primitive rocks, and among them we shall find that even granite itself is open to the attacks and inroads of one or other, and sometimes of all, the decom-

posing and destructive agents which we have enumerated. Some of its varieties are, it is true, extremely permanent, even in mountain masses, and seem almost imperishable, when considered in respect to their uses in the arts. Witness the mighty remains of the ancient Egyptian sculptors, executed in this hard and durable material; or look at the high state of preservation which some ancient temples, of a very remote date, present, and which still stand nearly perfect, amidst the fragments and dust of contemporary edifices, constructed of marble and other softer kinds of stone, as proofs and illustrations of these remarks. The beauty of granite, as a modern building material, may be seen in many of the public edifices of Dublin; and when we reflect upon the ease with which it may be brought into this metropolis from various parts of the British dominions, by water-carriage, one is involuntarily led to regret the continued use of Portland and Bath stone in some of the magnificent works now in progress. To these remarks however, Waterloo Bridge furnishes a splendid exception, and the material of which it is constructed, no less than the grand simplicity of the design, and the perfection of its execution, unite to constitute it a fit and durable public memorial of the genius and skill of its architect.

The decay of granite, considered in relation to its mountain masses, is effected in various ways, depending upon the structure and composition of the rock. In Cornwall, for instance, there are large patches of granite so soft and yielding, that it may be rubbed to powder between the fingers, and the grains of quartz and mica may be separated from the pulverulent felspar, which is the ingredient that undergoes decomposition, and forms a fine white powder, which is washed out of the rock by the rain and streams that percolate the district. A very striking assemblage of these decomposing granite hills is presented in an excavation near St. Austle, in Cornwall, known by the name of Carglaise Tin-mine. The veins of ore are harder than the rock itself, and easily traced upon its rapidly-decaying surface; and every rill of water that traverses the works is loaded with that finely-divided felspar, so abundantly deposited in all the lowlands of the neigh-

bourhood, and largely exported, under the name of Cornish or Porcelain Clay.

At the Land's-End the granite is of a very different character, and does not suffer that chemical change, which probably consists in the abstraction of the alkaline matter of its felspar, causing it to disintegrate in the manner just described; but there it splits into natural blocks, which are gradually removed from each other by the congelation of the water that gets between them; and where the surface is precipitous, they are thus often dislodged, and fall into the valley, or upon the beach beneath. The tendency of these square blocks to yield at their edges and angles to the weather, and thus to acquire a spheroidical appearance, independent of attrition by water, has already been adverted to in mentioning the granite Tors of Cornwall.

In other granitic districts, the structure of the rock is more decidedly lamellar, or stratified, and the direction of the fissures vertical, or nearly so. Sometimes they appear made up of successive layers, a texture seen in many of the Alpine peaks of Mont Blanc; and where this is the case, their decay produces that jagged and irregular appearance which we see, for instance, around the summit of Mont-Anvert, forming the *needles* of Chamonvi. When to these causes of destruction we unite the effects of those enormous accumulations of ice and snow, constituting the Glaciers of the Alpine regions; when we see these immense mountains, for such they are, composed of aggregated masses of rocks and ice, continually travelling into the valley, and, acted upon by the elevated temperature, thawing into rivers, or suddenly falling from their elevated stations, and destroying everything in their path, it is impossible not to be forcibly impressed with the great work of destruction which is thus carrying on. Indeed, we can scarcely pass an hour amongst the mountains that border Mont Blanc, without seeing or hearing an avalanche, falling with a noise that equals the most tremendous thunder-clap, from some of the neighbouring heights.

We have now considered the principal agents by which the disintegration and decay of rocks is effected; and have witnessed,

as the joint result of their operation, a continual wearing away of the strata, to such an extent, indeed, that, in the progress of years, some geologists have anticipated the entire disappearance of all irregularities upon the earth's surface. Thus the Huttonians would have us believe, that the countries that are now so beautifully diversified with hill and dale, and the mountains that shade and fructify the valley by their streams, are to be worn down into arid deserts—their materials are to be transported into the ocean—and the globe again become covered with the waters of the deep. And if we go back in our imagination to a former period of the earth, we may suppose that, since the same operations have been going on for ages, those great chains of mountains, whose present state we have just contemplated, are themselves but the remains of masses infinitely greater and more lofty; that perhaps they have been covered with other materials, and that these have been abundantly removed. Of such removal, it is presumed, we often have distinct evidence in the insulated masses of trap-rocks, which, if thrown up in a fused state into their present situation, must necessarily have been surrounded and supported by materials, the subsequent removal of which now exhibits the naked and insulated mass.

Such are at least some of Dr. Hutton's *principia*—his theory regards all the materials of our present strata as on their road to the deep—it allows of no exception to the law of decay. But as the Author of Nature has not given laws to the universe, which, like human institutions, carry with them the elements of their own destruction—as he has not permitted in his works any symptom of infancy or of old age, it was necessary in this theory to provide for reproduction, and such a provision Dr. Hutton has derived from the agency of submarine fire, by which the substances now accumulating in the depths of the sea, are to be again agglutinated and elevated. “From the land now going to decay,” says Mr. Playfair; “from the sand and gravel forming on the sea-shore, or in the beds of rivers—from the shells and corals which, in such enormous quantities, are every day accumulated in the bosom of the sea—from the drift-wood and the

multitude of vegetable and animal remains continually deposited in the ocean—from all these we cannot doubt that strata are now forming in these regions, to which nature seems to have confined the powers of mineral reproduction, from which, after being consolidated, they are again destined to emerge, and to exhibit a series of changes similar to the past.”

If, however, we examine the progress of this decay a little more closely, we shall, I think, discern several intermediate opposing agents, which the Huttonian theory overlooks, and shall perhaps be inclined to consider the changes that are going on, rather as tending to increase the habitable surface of the globe, to add to its beauty, to its fitness for the support of life, to convert uninhabitable and barren portions into fruitful and populous territories. Such, at least, has been their effect—such seems to be their present operation—and such their ultimate end. Let us, therefore, briefly inquire into the compensating and regenerating effects of the causes we have enumerated, independent of that ultimate reproduction at which the Huttonian theory points. In the first place, the formation of soils, by the washing down of finely-divided earthy particles from the mountainous land, and their accumulation in the plain, or valley, or upon the shelves and horizontal ridges of the hills, naturally attracts our attention as an important and beneficial consequence of this universal degradation of the globe. Of this finely-divided matter, it is true that a very great portion forms the mere mud of rivers, but where they are slackened in their progress, or where they widen out into lakes, they slowly deposit it upon their banks, increasing the extent of the neighbouring meadows, and diminishing their own boundaries. We see the troubled waters of the mountain-stream again issuing from the lake, pure and pellucid—we observe the lake slowly filling up, and its sandy accumulations, bound together by the reeds and grass upon its banks, resist the fluctuations of its waters, and soon become firm ground. Of this the lakes of Scotland and of Cumberland furnish abundant proofs, but the lake of Geneva presents us with the most grand and instructive illustration. That immense basin of fresh water is, like all other

lakes, gradually diminishing in mean depth ; the Rhone, with its countless tributary streams, is seen pouring its abundant waters, turbid with the débris of the great Alpine chain of the Valais, into the eastern end of the lake ; there the river is always loaded with finely-divided matter, and often muddy and opaque, but when it again issues from the opposite extremity, its waters are of the brightest hue ; and as it hurries its divided streams through the streets of Geneva, the observer's attention is always arrested by the peculiar blue tint which announces its extreme purity. This is the history of all lakes, and these again are but the types of the ocean itself—that great reservoir, into whose depths all the impurities of the land-streams are ultimately consigned, and from which the waters are again raised in renovated purity, to return to the earth in fertilizing showers, or to be condensed into streams, and accumulated into rivers and lakes by the summits of hills and mountains. What may be the effect of various and contending streams in the bosom of the deep, or what the exact arrangement and final distribution of the matter carried into it—to what new agencies it may there be liable, or to what modifications of the known powers of matter, can scarcely be surmised ; but we know thus much for certain, that no inconsiderable portion of it is thrown up in the form of bars and sandbanks, at the mouths of rivers—that shallows are formed upon coasts—that it is frequently agglutinated upon flat and shelving shores into new strata—and that much is modified by the incessant labours of zoophytic animals into coral and madrepores, of which enormous shoals, and islands, and even parts of continents, are visibly formed, and of which there no doubt exist enormous and unknown accumulations in hitherto-unexplored regions. In short, the aggregate effects of the labours of these microscopic animals surpass all calculation. They probably commence their work upon some submarine rock, and it is continued by successive races till the enormous wall of coral reaches the surface. The care to raise it perpendicularly shows, says Captain Flinders, a remarkable instinct in these diminutive creatures ; and being generally built where the winds are constant, it affords a shelter, upon the leeward of which their infant colonies may safely be

sent forth. To this instinctive foresight it seems to be owing that the windward side of a coral-reef, exposed to the open sea, is generally the highest part, often rising from the depth of some hundred fathoms. These animals always work under water, but the sand and broken remnants thrown up by the sea adhere to the reef, and form a solid mass as high as the tides reach. The new bank is then visited by gulls and other sea-birds; marine vegetables next take root upon it, and a soil begins to be formed. Perhaps a cocoa-nut is thrown up by the water, or land-birds visit it, and deposit the seeds of shrubs and trees. It thus gradually assumes the form of a verdant island, and, lastly, man becomes its possessor.

Thus we see that if, on the one hand, destructive agents are continually at work, renovation is also going on; sometimes by mechanical operations, and at others by the labours of various animal tribes, regeneration of surface is effected to a remarkable and unexpected extent. To this end the vegetable creation is also conducive, and when, by some of the operations which we have considered, a stratum of finely-divided matter is lodged upon a rocky surface, it is detained there, and matted together by the growth of various plants, which thus not only detain the soil itself, but incrust and protect the rock which supports it, from the various agents of degradation that are at work upon its naked neighbour: heath attaches vegetation to a sandy soil; moss fixes it upon a rocky surface, and herbs and brambles interweave their roots and render it permanent. Under these circumstances, as soon as decomposition of a rock commences, asperities are softened down, and steep and abrupt precipices gradually converted into regular and gentle slopes—the seeds of lichens, which are constantly floating in the air, make these their resting-places—their generations occupy it till a finely-divided earth is formed, capable of supporting moss and heath. Acted upon by light and heat, these plants drink in the dew, and convert the constituent parts of the atmosphere into nourishment. Their death and decay afford food for a more perfect species of vegetable, and at length a mould is formed, in which even the trees of the forest fix their roots, and which is capable of rewarding the labours of the cultivator.

ART. V. *On Improvements in the Solution of Equations, by Continued Fractions.* By W. G. HORNER.

[Continued from Vol. XXI.]

III.

25. The solution of the general case $x^n - L = 0$ will yield a fair criterion by which to estimate the comparative merits of the method of correct average, and that of extreme limits, to which Lagrange adheres. This individual case does not favour one mode rather than the other, and it is that which our author has exemplified in numbers.

Here the general formula in Art. 21 becomes

$$F_x = \frac{\rho^{n-2} + 2\xi^{n-3}\rho'x + 3\xi^{n-4}\rho'^2x^2 + \dots + (n-1)\rho'^{n-2}x^{n-2}}{(n-1)\rho^{n-2} + (n-2)\rho^{n-3}\xi'x + \dots + 2\xi\rho'^{n-3}x^{n-3} + \xi'^{n-2}x^{n-2}} \times x \dots [1$$

a quantity which is necessarily affirmative, implying that $\frac{\pi'}{\xi'}$ and $\frac{\pi}{\xi}$ are, from the very outset, limits to the average of the imaginary roots. (Art. 19.)

26. This expression gives at once

$$F_r = \frac{\xi}{\xi'}$$

which, in powers not exceeding the fourth, is a sufficiently close approximation, even at the outset, to obviate any impediment.

But if for x^{n-2} in the final terms of F_x , we substitute its equivalent $\frac{L}{x^2}$, on the principle of Art. 21, we shall then find

$$F_r = \frac{\frac{1}{2}(n-1)(n-2)\xi^n + (n-1)L\rho'^n}{\frac{1}{2}(n+1)(n-2)\rho^n + L\xi'^n} \times \frac{\xi}{\rho'}$$

which is so close an approximation, that even in the tenth power it leaves no obstruction.

For the most unfavourable circumstances possible are those in which $\frac{\xi}{\xi'}$ and x are most disproportionate, and the extreme of all

these occurs when $\frac{\rho}{\rho'} = 1$ and $x = 1.5$. With these data we

have in the fourth power $F_x = \frac{43}{22}$, which gives, when $F_r = \frac{\xi}{\xi'}$

$$E_1^7 = \frac{63}{130} < \frac{1}{2};$$

and in the tenth power, $F_x = \frac{1627}{413} = 4$ nearly, and F_r by the

second formula $= \frac{279}{51} = 5 \frac{1}{2}$ nearly, giving

$$E_1 = \frac{27}{65} < \frac{1}{2}.$$

For every practical purpose, then, the second formula is abundantly satisfactory; but the first may be used with confidence whenever L is more than a unit or two removed in either direction from $\left(\frac{3}{2}\right)^{n>4}$ or $\left(\frac{5}{2}\right)^{n>7}$, &c.; viz. within a unit or two of

8 in the fifth root, 11 in the sixth, 17 in the seventh, 26 and 1526 in the eighth, &c. In these situations, we may either resort to the second formula, or solve the equation $(2x)^n - 2^n L = 0$, which is equivalent to the given one.

Lagrange fixes the limits of certainty in his mode of operating at $L > 12$ in the third power, or 256 in the fourth, or 1315 in the fifth, or 110592 in the sixth, &c.

27. Putting $(1 + 1)^n = 1 + \alpha + \beta + \gamma + \dots$, and $u = x - \frac{\xi}{\rho'}$

$= \frac{\mp 1}{\rho'(\pi' + \xi't)}$, we obtain.

$$F_x = \frac{\beta \xi^{n-2} + 2\gamma \xi^{n-3} \xi' u + 3\delta \rho^{n-4} \rho'^2 u + \dots (n-1) \xi'^{n-2} u^{n-2}}{\beta \rho^{n-2} + \gamma \xi^{n-3} \xi' u + \delta \xi^{n-4} \xi'^2 u + \dots \xi'^{n-2} u^{n-2}} \times x \dots [2]$$

which, by actual multiplication and division, yields

$$F_x = \frac{\rho}{\rho'} + \frac{n+1}{3} u + \frac{n+1}{3} \cdot \frac{n-2}{6} \cdot \frac{u^2 \rho'}{\xi} + \frac{n+1}{3} \cdot \frac{n-2}{6} \cdot \frac{n-8}{15} \cdot \frac{u^3 \xi'^2}{\xi^2} + \&c. \dots [3]$$

formulæ more convenient than the first for pursuing the solution.

28. If we revert to the manner in which F_x is engaged in t_x

(Art. 23); and to the general relations of f in Art. 17, it will be apparent that F_x should be, and may be immediately, reduced to the form of a continued fraction. For, since its value varies but little from $\frac{\xi}{\xi'}$, we have only to assume $F_x = \frac{\xi + \pi z}{\xi' + \pi' z}$, whose expanded value will differ from that of $\frac{\rho}{\xi}$ only in the addition of z to the final denominator.

29. To obtain the value of z , we first observe that $F_x - \frac{\xi}{\rho}$, by the last article, is $= \frac{\mp z}{\rho'(\xi' + \pi' z)}$. But putting $\frac{Nx}{D}$ as equivalent to the second form of F_x , and $(1 + 1)^{n+1} = 1 + \alpha_1 + \beta_1 + \gamma_1 + \dots$, we have $F_x - \frac{\xi}{\rho} = \frac{Nx - D \frac{\xi}{\rho}}{D}$; where the numerator is equal to $\gamma_1 \rho^{n-2} u + 2\beta_1 \xi^{n-3} \xi' u^2 + 3\alpha_1 \xi^{n-4} \xi'^2 u^3 + \dots (n-1) \xi'^{n-2} u^{n-1} =$ say to Mu , that is $\frac{\mp M}{\rho'(\pi' + \xi' t)}$.

Hence the equation

$$\frac{z}{\xi' + \pi' z} = \frac{M}{(\pi' + \xi' t) D}$$

which produces

$$z = \frac{M \rho'}{(\pi' + \rho' t) D - M \pi'} \dots \dots \dots (1)$$

In the second power the value of this quantity is merely $\frac{1}{t}$,

but in the third power it becomes
$$\frac{2(2t + \frac{\pi}{\rho} + \frac{\pi'}{\xi'})}{(3t + \frac{\pi}{\rho} + \frac{\pi'}{\xi'}) t - \frac{\pi \pi'}{\xi \xi'}}$$

and in higher powers increasingly complicated in respect of the powers of t . Of course, in its general condition it appears incapable of being expanded into a continued fraction; and its application, even in the way of reiterated approximation, is more laborious than it would be to carry the process of direct transforma-

tion by equations a few steps farther at commencing. I shall therefore, in this place, only observe, that the third form of F_x , if we reject the terms involving powers of the small fraction u , gives the approximate value

$$z = \frac{(n+1)\rho'}{3\varepsilon't - (n-2)\pi'} \dots \dots \dots (2)$$

which, being $> 0 < 1$ when t is $> \frac{n+1}{3} + \frac{n-2}{3} \cdot \frac{\pi'}{\varepsilon'}$, is sure to be a fraction, if the transformations be stopped at an equation whose root is not very small.

30. We now have, on substituting the value of F_x (Art. 28) in that of t (Art. 23),

$$t_x = a + \frac{(n-1)(\pi'\varepsilon' + \pi\varepsilon' + 2\pi\pi'z)}{2\rho\rho' + (\pi'\rho + \pi\rho')z}$$

which expanded and substituted for t in the original, or Lagrange's expression for x , gives

$$x = p + \frac{1}{p_1} + \frac{1}{p_2} + \dots + \frac{1}{p_r} + \frac{1}{a} + \frac{n-1}{p_r} + \frac{1}{p_{r-1}} + \dots \\ \frac{1}{p_1} + \frac{1}{2p} + \frac{1}{p_1} + \dots + \frac{1}{p_r} + z.$$

And this expression, if z be neglected, is manifestly more than triple of the original basis $p + \dots + \frac{1}{p_r}$ in extent; which was one point to be proved.

31. The advantage of this method above the original is, in some instances, much greater than I have here asserted. One more striking need not be sought for, than that with which our author concludes his treatise, viz. the cube root of 17. This he determines to be $2 + \frac{1}{1 + \frac{1}{2 + \frac{1}{-2 + \dots}}} = 2, 3, \frac{8}{3}, \frac{-13}{-5}, \&c.$ The

defective state of this result is remarkable. The latter aggregates are not even principal fractions in the converging series, and the leading terms of the continued fraction are both of them erroneous; that is, they are not, what they were designed to be, the most accurate correction of the preceding terms respectively.

Accuracy might have been secured by adopting the amendment given in Art. 24 of these papers. Rating F_x at its rudest value

$\frac{\rho}{\rho'}$, $E_{2\rho'}$ would be $= \frac{n-1}{2\rho}$, $= \frac{1}{\rho}$ in the instance of the cube

root; which should be deducted from $R = \frac{3\rho^2}{\rho^3 - L\rho'^3}$ at each step,

or, in other words, R should be accounted $= \frac{2\rho^3 + L'\rho^3}{\rho(\rho^3 - L\rho'^3)}$. It would

then be found that the first denominator should not be 1 as the nearest integer to $\frac{4}{3}$, but 2 as nearest to $\frac{11}{6}$; and that if 1 were

adopted, it should not be followed by 2 as nearest to $\frac{17}{10}$, but by 1

as nearest to $\frac{41}{30}$, which are the more correct values of $\frac{\pm R - \pi'}{\rho'}$.

In short, at as many steps as our author has employed we should

have found the root to be $2 + \frac{1}{2} + \frac{1}{-4} + \frac{1}{-138}$. And since the

general equation of the quantities which he denotes by a and R ,

is readily found to be $a = \frac{\pm R - n\pi'}{\rho'}$, which at the last step of the

above process is $= \frac{-138}{1}$, the extended root, Art. 30, is

$$2 + \frac{1}{2} + \frac{1}{-4} + \frac{1}{-138} + \frac{2}{-4} + \frac{1}{2} + \frac{1}{4} + \frac{1}{2} + \frac{1}{-4+z} =$$

$$2 + \frac{1}{2} + \frac{1}{-4} + \frac{1}{-138} + \frac{1}{-2} + \frac{1}{4} + \frac{1}{2} + \frac{1}{4} + \frac{1}{-2} + \frac{z}{2}$$

where the approximate value of $\frac{z}{2}$ is $\frac{1}{\frac{3}{2}(-138\frac{71}{126}) + \frac{1}{7}} =$

$\frac{1}{-207\frac{59}{84}}$. If the root thus corrected were reduced to decimals

it would be found correct in the 17th decimal place ; while the value, at which the original operation terminated, is barely correct in the 1st decimal.

Restoring the affirmative character, the root we have found is

$$2 + \frac{1}{1} + \frac{1}{1} + \frac{1}{3} + \frac{1}{138} + \frac{1}{1} + \frac{1}{1} + \frac{1}{3} + \frac{1}{2} + \frac{1}{3} + \frac{1}{1} + \frac{1}{1} + \frac{1}{207} \\ + \frac{1}{1} + \frac{1}{2} + \frac{1}{2} + \frac{1}{1} + \&c.$$

correct in every term.

32. Several circumstances, including the casual magnitude of a , have contributed to the easy attainment of so accurate a result: it may be well to examine if the rest are also casual. The integral form of a need not be insisted upon, as the presence of a fractional part in one denominator would but slightly interfere with the rigid conclusions of Lagrange, and may be suffered, for other reasons to be touched upon presently.

The ready extermination of the irregular numerator 2, was owing to the presence of *even* denominators in that and the succeeding *alternate* places ; for if these and the *even* numerator be halved, and the remaining denominators doubled, it is sufficiently clear that the fraction remains unaltered in value. Since the alternate denominators can, in every case, be made even numbers by introducing negative terms when requisite ; [See *Res. des Eqq. N.* § 67, and Add. to Euler's *Alg.*, § 7 ;] this reduction, as far as the cube root is concerned, is not accidental, but may always be performed without aggregating the series.

33. Lagrange has marked it as a desideratum in this branch of algebra, to find some characteristic peculiarities which should serve to distinguish the suites of fractions derived from commensurable divisors of the third and higher degrees. Some indications of such a characteristic, adhering to the binomial forms considered in this paper, are apparent in the expanded value of x , which, as the result of the numerical example shows, are not always obliterated by exterminating the irregular numerator $n-1$.

The characteristic alluded to will be best explained if we commence with the second root. Here z is accurately $= \frac{1}{t}$; so that

at every transformation the root is accurately determined; a fact hitherto, I believe, unobserved. For example,

$$\sqrt[3]{73} = 8 + \left(\frac{1}{16} + \frac{1}{16} + \right)^{\infty} =$$

$$8 + \frac{1}{1} + \left(\frac{1}{2} + \frac{1}{8} + \frac{1}{16} + \frac{1}{1} + \right)^{\infty} =$$

$$8 + \frac{1}{1} + \frac{1}{1} + \left(\frac{1}{14} + \frac{1}{3} + \frac{1}{1} + \frac{1}{16} + \frac{1}{1} + \frac{1}{1} + \right)^{\infty} = \&c.$$

the general form being $R + \left(\frac{1}{p_1} + \frac{1}{p_2} + \dots + \frac{1}{a} + \dots + \frac{1}{p_2} + \frac{1}{p_1} + \frac{1}{R} \right)^{\infty}$ conformably with the theorems in Art. 13, which may indeed be deduced immediately from this.

In higher roots, on account of the inextricable form of z in its accurate condition, an approximation only to the correct value is obtained at every transformation. Thus

$$\sqrt[3]{17} = 2 + \frac{1}{12} + \frac{2}{9} + \frac{2}{4} + z =$$

$$2 + \frac{1}{1} + \frac{1}{-3} + \frac{2}{10} + \frac{2}{1} + \frac{1}{4} + \frac{1}{1} + z_1 =$$

$$2 + \frac{1}{1} + \frac{1}{1} + \frac{1}{21} + \frac{2}{11} + \frac{2}{1} + \frac{1}{4} + \frac{1}{1} + \frac{1}{1} + z_{11} = \&c.$$

34. In every power, the reciprocating arrangement of the denominators between a and z is observed. Respecting this portion only, a few remarks are yet to be made.

When z , abstracting the sign of t where requisite, is affirmative or $> 0 < \infty$, which is sure to be the case when $t > \frac{n-2}{3} \cdot \frac{\pi'}{\rho'}$,

we have (see t_x in Art. 30.)

$$\frac{t-a}{n-1} > \frac{2\pi\pi'}{\pi'_e + \pi'_p} < \frac{\pi'_p + \pi'_e}{2e e'}, \text{ say } > \frac{H'}{K'}, < \frac{H}{K};$$

but the difference of these fractions has for its numerator,

$$(\pi'_p + \pi'_e)^2 - 4\pi\pi'\rho\rho' = 1.$$

Consequently they are consecutive fractions in a series converging toward $\frac{t-a}{n-1}$.

And because $\frac{H'}{H}, \frac{K'}{K}$ are identical with $\frac{H'}{K'}, \frac{H}{K}$, the continued fraction will have the same course of denominators backward as forward, agreeably to what was stated in Art. 8.

Now suppose $n-1$ to be a divisor of $2\epsilon\epsilon'$, and put $Q =$ the quotient. Then will

$\frac{(n-1)2\pi\pi'}{\pi'\rho + \pi\epsilon'}$ and $\frac{\pi'\rho + \pi\epsilon'}{Q} =$ say to $\frac{P'}{Q}, \frac{P}{Q}$, be consecutive fractions; for they are limits to $t-a$, and their difference has the same numerator as before, viz., 1.

And because $\frac{P'}{P}, \frac{Q'}{Q}$ are identical with $\frac{P'}{Q'}, \frac{P}{Q}$, the continued fraction will here also be of the reciprocating form.

Whenever therefore $n-1$ is a divisor of $2\epsilon\epsilon'$, the reciprocal form will remain when the irregular numerator $n-1$ is exterminated.

When however $2(n-1)\pi\pi' > \frac{2\epsilon\epsilon'}{n-1}$, or $(n-1)^2\pi\pi' > \epsilon\epsilon'$, the first denominator of the reduced continued fraction will not be repeated. For we calculate on $\frac{P}{Q}$ as the final aggregate; whereas, in the case now named, $\frac{P'}{Q'}$ being $> \frac{P}{Q}$, is the aggregate of the entire reciprocal series.

35. The only instance in which $n-1$ will everywhere measure $2\epsilon\epsilon'$ is when $n=3$: With regard to this root therefore, it will be found that whenever a , the quotient of the second term of a transformed equation by the first with its sign changed, is a positive integer, the continued fraction will, to a finite extent from that point, be of the reciprocal form; and, because of the known value of z in terms of t , the root of the transformed equation, which immediately follows this reciprocal portion, will be to the root of that which preceded the same, in the ratio of 3:2 very nearly. This property is very apparent in the root of 17 which we have given in Art. 31.

If a is not restricted to integral values, such reciprocal portions as the above will occur, whenever twice the product of the terms of that simple fraction, which is the aggregate of the continued fraction actually determined, is divisible by $n-1$; in which event the characteristic ratio $n+1:3(n-1)$ nearly, will obtain between the roots, if considerable, of the transformed equation immediately preceding and following. If more definite and complete criteria are desirable, they can only be obtained by a further developement of the function z . But I desist.

IV.

36. There can be no doubt, that the system of resorting to compendious formulæ, as a substitute for the direct and complete transformation of equations, has been a frequent source of errors and oversights in works on algebra. Those which have occurred to my notice in the celebrated treatise, of some portions of which I am venturing to offer amendments, can be referred, in most instances, to no cause so probable as this; and the detection of them was the natural consequence of restoring transformations for compendia, in reading the author. As these transformations were not conducted *more majorum*, but on a plan whose principles I had the honour of explaining in the *Philosophical Transactions* some years ago, this mode of interpretation proved to be as much recommended by facility, as it might have been expected to be by luminousness.

For example, the extraction of the cube root of 17, discussed above, is entirely comprized in the few figures following:

$$\begin{array}{r}
 1 \quad 0 \quad 0 \quad -17(2 = p \\
 \quad 2 \quad 4 \quad 8 \\
 \hline
 \quad 2 \quad 4 \\
 \quad 2 \quad 4 \\
 \hline
 \frac{12}{9} + \frac{2}{2p} > p_1 = 1) \quad 1 \quad 6 \quad 12 \quad -9 \\
 \quad 10 \quad 3 \quad 15 \quad -9(1 = p_2 < \frac{-3}{10} + \frac{2}{p_1} + \&c. \\
 \frac{21}{11} + \frac{2}{p_2 + \&c.} > p_3 = 3) \quad 10 \quad 33 \quad 21 \quad -11 \\
 \quad -9 \quad -36 \quad -33 \\
 \quad \quad -3 \quad -12 \\
 \quad \quad -99 \quad -33 \\
 \hline
 \quad \quad 1-138 \quad -78 \quad -11(\therefore a = 138
 \end{array}$$

$$\begin{array}{ccccccc} \text{Then} & 2 & 1 & 1 & 3 & & \\ & \frac{1}{0} & \frac{2}{1} & \frac{3}{1} & \frac{5}{2} & = \frac{\pi}{\pi'} & \frac{18}{7} = \frac{\xi}{\rho'} \end{array}$$

whence $t = 138 \frac{71}{126} = 138 + \frac{1}{1} + \frac{1}{1} + \frac{1}{3} + \frac{1}{2} + \frac{1}{3} + \frac{1}{1} + \frac{1}{1}$
omitting z .

$$\text{And } z = \frac{1}{\frac{3}{2}t - \frac{\pi'}{\rho'}} = \frac{1}{207} + \&c.$$

which directly conducts to the final formula in Art. 31. The marginal calculations comprize as much as is necessary of the progressive formulæ for t_x in Art. 30. And there is nothing in the transformations which is not well known, or at least easily to be understood and verified by the eye.

37. As the solution of Quadratic Equations, which after all is the most interesting department of the theory, assumes, under this management, a character of the utmost neatness and fluency, I may be permitted to explain it fully in this place; which may be done very concisely too, on account of the preceding general deductions.

The general conduct of the transformations will be clear, from the following formula:

$$\begin{array}{c} a - b + a_1 \left(p < \frac{d}{a} \right. \\ [d] \quad \frac{ap}{b_1} \quad b_1 p \\ \hline \left. \frac{d_1}{a_2} > p_1 \right) a_3 - b_2 + a_2 \end{array}$$

where $a_3 = a$, and $b_1, -b_2, a_2$ are the respective sums of the two quantities next above them $[d]$, and the dividends d, d , &c. are thus found.

38. The value of p , the first portion of the root, being sought in the usual manner, is the integer

$$< \frac{\frac{1}{2} (b \pm \sqrt{b^2 - 4aa_1})}{a}, \text{ say } < \frac{\frac{1}{2} (b \pm r)}{a}$$

In like manner p_1 is the integer $< \frac{1}{2} \frac{(b_2 \mp r)}{a_2}$ and so forward.

Wherefore, if we put $d = \frac{1}{2} (b \pm r)$ and $d_1 = \frac{1}{2} (b_2 \mp r)$ we have $d + d_1 = \frac{1}{2} (b + b_2)$. But by the general formula $b_1 = -b + ap$ and $-b_2 = b_1 + ap$; whence we find $-b_1 = \frac{1}{2} (b + b_2) = d + d_1$. Consequently $d_1 = -(b_1 + d)$.

Having therefore, in the first instance, determined the integer d , which is next $< \frac{1}{2} (b \pm \sqrt{b^2 - 4aa_1})$ the dividends for the successive equations are derived from each other successively, by adding the undermost term used in the transformation, and changing the sign of the sum. The dividend may most conveniently be placed under its divisor and immediately crossed with the pen, or bracketted, to prevent mistakes in the subsequent additions.

39. If both values of d be affirmative, it is of no consequence which of them is adopted. After a finite number of transformations, we shall arrive at an equation, whose extreme terms are of contrary signs, while the sum of the mean and either extreme, abstracting the signs, is $>$ the other. The portion of the root previous to this point will be common to both roots of the proposed equation*; the remaining portion will be periodical. Let the whole be

$$p + \frac{1}{p_1 + \dots + p_m} + \left(\frac{1}{q} + \frac{1}{q_1} + \dots + \frac{1}{q_r} + \right)^\infty$$

the other root of the equation* will be

$$p + \frac{1}{p_1 + \dots + p_m - q_r - q_{r-1}} + \left(\frac{1}{q_{r-2}} + \dots + \frac{1}{q} + \frac{1}{q_r} + \frac{1}{q_{r-1}} + \right)^\infty$$

And the negative sign may be effaced by the use of the known formulæ

* This is equally true with respect to any subsequent point of the transformations, but several periodical fractions would then destroy each other, and the infinite result remain in every instance the same as we have given it.

$$u - \frac{1}{v} = u - 1 + \frac{1}{1} + \frac{1}{v-1} \text{ and}$$

$$u - \frac{1}{1} + \frac{1}{v} = u - 1 + \frac{1}{v+1}.$$

This follows from the principles of Art. 11, 12.

EXAMPLE. Let it be required to find the roots of the equation $9x^3 - 118x + 378 = 0$, cited *ubi supra*.

Here $d = 59 \pm \sqrt{79} = 67$ or 50 . Employing the second value, we proceed thus:

$\begin{array}{r} 9 - 118 + 375 \quad (5) \\ (50) \quad \underline{45 - 365} \\ \quad \quad -73 \\ \hline 1) \quad 9 - 28 + 13 \\ \quad \underline{-15 \quad 13} \quad (22) \\ \quad \quad -15 \\ \hline \quad \quad -6 \quad -2 + 13 \quad (1) \\ (-7) \quad \quad \underline{-6 \quad -8} \\ \quad \quad \quad -8 \\ \hline 3) \quad -6 \quad -14 + 5 \\ \quad \quad \underline{3 \quad 15} \quad (15) \\ \quad \quad \quad 1 \\ \hline \quad \quad -3 \quad +16 + 5(3) \\ (-16) \quad \quad \underline{-15 \quad 5} \\ \quad \quad \quad 1 \\ \hline 1) \quad -3 \quad -14 + 10 \end{array}$	$\begin{array}{r} 1) \quad -3 - 14 + 10 \\ \quad \quad \underline{-4 \quad 10} \quad (15) \\ \quad \quad \quad -4 \\ \hline \quad \quad -7 + 6 + 10 \quad (1) \\ (-11) \quad \quad \underline{-7 - 1} \\ \quad \quad \quad -1 \\ \hline 1) \quad -7 - 8 + 9 \\ \quad \quad \underline{1 \quad 9} \quad (12) \\ \quad \quad \quad 1 \\ \hline \quad \quad -6 + 10 + 9 \quad (2) \\ (-13) \quad \quad \underline{-12 - 4} \\ \quad \quad \quad -2 \\ \hline q=3) \quad -6 \quad -14 + 5 \\ \quad \quad \quad \quad \quad (15) \end{array}$
---	--

Hence the inferior root is $5 + \frac{1}{1} + \frac{1}{1} + \left(\frac{1}{3} + \frac{1}{5} + \frac{1}{1} + \frac{1}{1} + \frac{1}{1} + \frac{1}{1} + \frac{1}{2} + \right)^\infty$. Consequently the greater is $5 + \frac{1}{1} + \frac{1}{1-2} - \frac{1}{1} + \left(\frac{1}{1} + \frac{1}{1} + \frac{1}{5} + \frac{1}{3} + \frac{1}{2} + \frac{1}{1} + \right)^\infty$, where the portion previous to the period, viz. $5 + \frac{1}{1} + \frac{1}{-1} - \frac{1}{-1}$ is readily found

to be $= 7$. This accords with the statement in Art. 12.

40. From the same principle, combined with the known properties of reciprocal equations, we readily derive the following theorems: (1). If in the periodical series of equations, we arrive at one whose *extreme coefficients are equal*, the succeeding equation will be the negated reciprocal of the *preceding* one, and the denominators already found, *including the last*, will return in reverse order. (2.) If the *first coefficient measures the second*, the quotient is the correct denominator [for the second term in $t = a + \frac{\pi + \pi'x}{\rho + \rho'x}$ is necessarily fractional], the succeeding equation will be the negated reciprocal of the *existing* one, [because b_1 being $= 0$, b_2 is $= ap = -b$, &c.] and the denominators already found, *exclusive of the last*, will return in retrogradation.

In each event, the actual evolution of these denominators may be superseded, and the series continued, by returning at once to the negated reciprocal of the leading equation in the period, and its original dividend, also with the sign changed.

As reciprocal equations can only be connected in these two ways, and we have seen that every square root is of the reciprocating form, the middle point in the evolution of every square root is sure to be indicated by the occurrence of one or the other of these events.

Ex. The square root of $\frac{11}{3}$

$$\begin{array}{r} \sqrt{33} > (5) \quad \frac{3 + 0 - 11}{3} \quad (1) \\ \hline q = 1) \quad \frac{3 + 6 - 8}{-2 - 8} \quad (-8) \\ \hline \quad \quad \quad -2 \\ \hline \quad \quad \quad 1 - 10 - 8 \quad (10, 1, \frac{6}{3}) \end{array}$$

$\therefore$ the root is $1 + \left(\frac{1}{1+10} + \frac{1}{1+2} + \dots \right)^\infty$

41. When r , the irrational portion of the root, is imaginary, it cannot affect the variations of the real portion of the dividend.

Consequently d , the first dividend, is simply $\frac{1}{2}b$, and so on. In no other respect does the evolution of the root, as far as is possible in affirmative fractions, differ from what has already been exemplified. Wherefore the preparations and trials employed by Lagrange (Add. to Eul. Alg. § 32) in determining the integers, p, q , which produce the minimum value of $Ap^2 + Bpq + Cq^2$ when $\sqrt{B^2 - 4AC}$ is imaginary, are superfluous. The regular course of transformations will indicate the point at which the *smallest absolute term* occurs, which will necessarily be either in the last equation with changes of sign, or the first with all signs alike. Thus, for the quantity $49p^2 - 238pq + 290q^2$, we have

$$\begin{array}{r}
 49 - 238 + 290 (2) \\
 (119) \quad \quad 98 - 280 \\
 \quad \quad \quad - 140 \\
 \hline
 2) 49 - 42 + 10 \\
 \quad - 44 \quad 20 \quad (21) \\
 \quad \quad \quad - 22 \\
 \hline
 \quad \quad 5 - 2 + 10 (0) \\
 (1)
 \end{array}$$

Here the least value is 5, $\frac{p}{q}$ being $= 2\frac{1}{2} = \frac{5}{2}$, or $p=5, q=2$ the same as found by our author.

And for the quantity $36p^2 - 170pq + 201q^2$, we have

$$\begin{array}{r}
 36 - 170 + 201 (2) \\
 (85) \quad \quad 72 - 196 \\
 \quad \quad \quad - 98 \\
 \hline
 2) 36 - 26 + 5 \\
 \quad - 32 \quad 10 \quad (13) \\
 \quad \quad \quad - 16 \\
 \hline
 \quad \quad 4 - 6 + 5 (1) \\
 (3) \quad \quad \quad 4 - 2 \\
 \quad \quad \quad \quad - 2 \\
 \hline
 \quad \quad 4 + 2 + 3
 \end{array}$$

The least value is 3, produced by $2 + \frac{1}{2} + \frac{1}{1} = \frac{7}{3}$ or $p=7, q=3$.

If this course of remarks has served to render the practice of an interesting department of analysis somewhat more consistent, clear, and easy, the writer trusts that the minuteness of a few of the details will meet with a ready excuse.

Bath, 28th Aug., 1826.

ART. VI. *On a peculiar Perspective Appearance of Aërial Light and Shade.* By M. F.

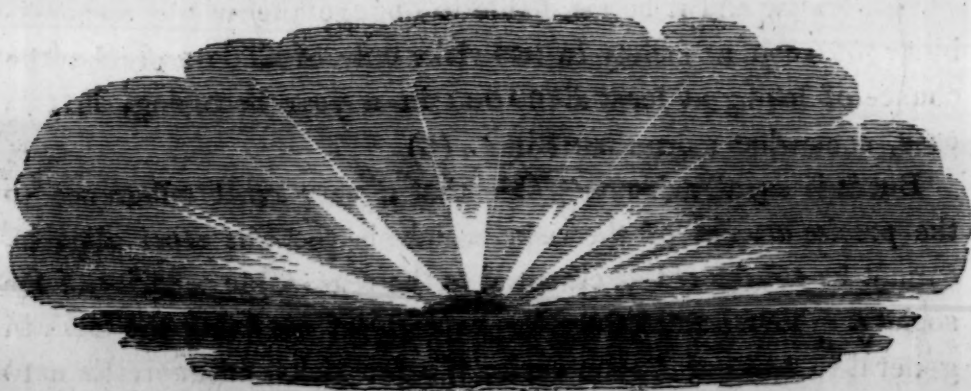
ONE evening last month (Aug. 19, 1826), a curious aërial phenomenon was observed from the undercliff at the back of the Isle of Wight, just above Puckaster Cove. The sky was clear, the sun had just set to those who were standing where the appearance was observed, when several enormous rays of light and shade were remarked towards the E., N.E., and S.E., all radiating in strait lines from a spot rather south of east, and just upon the horizon. They were ten or twelve in number, did not join at the place from whence they appeared to originate, but seemed to emerge from an obscure portion of surface of a convex form 8° or 9° in horizontal extent, and about the third of that in height. The rays extended from 30° to 40° on the right and left from the centre, but were of less extent as they became more vertical. They diminished gradually in intensity at the extremities until they could be traced no farther. The appearance slowly faded away, some of the rays disappearing before others, but was observed upon the whole for about half an hour.

At first the phenomenon seemed inexplicable, but after a little consideration, was referred (and as it appeared from after observations correctly) to an effect of aërial perspective. The rays which seemed to originate from a common centre on the east, were really only the intervals between long shadows caused by the occurrence of clouds far to the west, and were in fact passing to the place from whence they *seemed* to originate, and the circumstances of the case seem to have been as follow:—the at-

mosphere contained a slight haze, which allowed the sun's beams to pass forward with but little interruption, but was yet in sufficient quantity to reflect a considerable portion of light to the eye. The sun was just setting; the clouds very far to the west, and out of sight from the place where the observer stood, stopped the light wherever they interfered, and cast immense horizontal or nearly horizontal shadows along the sky, parallel to each other, and over the head of the observer. The difference between these shadows and the intervening illuminated parts, could not be observed over head, or on the right or left hand, i. e., perpendicular to their direction, because of the want of sufficient depth, as it were, in the parts thus circumstanced, to make them visible; but as they receded from the observer in the direction from the sun, they became fore-shortened, and then from the greater depth of mass, and consequently greater number of particles looked at, became visible. This is at least one reason why they were so visible towards the east; but another is the probable existence of more haze in that direction than towards the west, or to the right or left of the observer's situation: the rays could not be seen between the sun and the observer, though the sun was out of sight, and consequently the general light, it may be supposed, not too great; which seems to imply that less haze existed in that direction; and its presence was fully proved towards the east by the dull red colour which the moon assumed upon rising a short time after the appearance had ceased. The convergence of the rays to one spot, and that opposite the sun, was merely an effect of perspective, and requires no explanation here.

Although the appearance on this evening was exceedingly beautiful and rare, and the more striking from the absence, to the observer, of the sun or clouds, and the complete insulation of the phenomenon, yet by close observation upon other evenings, it was found that partial effects of the same kind were very common, and from the manner in which these could be observed, the explanation above given fully confirmed. On several evenings after, when observing the sun set from the neighbouring hill of St. Catherine, it was found that if the atmosphere was generally

clear, but with compact and distinctly-formed clouds floating in it, the effect was produced. The usual appearance of rays at sun-set, diverging amongst the clouds in the west, from the sun, is well known ; but even when these were not visible, upon looking to the eastern half of the hemisphere, and especially to the north or south of east towards the horizon, it was rarely that some clouds could not be distinguished, with long shadowy projections behind them, always converging to the spot opposite to the sun. Frequently, clouds could be selected moving more immediately in the neighbourhood of the observer : of those which passed overhead, the shadows could not be observed close to the clouds ; but carrying the eye onwards towards the east, the same shadows became visible, when considerably fore-shortened, and could be observed moving on and changing with the clouds themselves. All these phenomena, with their variations, were easily referrible to their causes, and may be observed at almost any sun-set in fine weather ; but the effect of the first evening, so similar in kind, though so different in appearance, was not again remarked. It is with a view of guarding persons who may observe the same effect, against any mistake as to its origin, that the appearance, with its nature, has been thus particularly described.



ART. VII. PRACTICAL Application of the DOCTRINE of CHANCES, as it regards the SUBDIVISION of RISKS.

[By a Correspondent.]

It is well known to those who have studied the theory of chances, that where the magnitude of a risk may be divided into an indefinite number of parts, it is possible to confine the probability of the occurrence of any given excess or deficiency in the result, above or below its mean value, within any given limits; and it is of great practical importance to ascertain, both how far the subdivision ought to be carried, with regard to insurances, and what are the inconveniences to be apprehended, from the admission of occasional deviations from the general rule.

We may take, for example, the case of an office having undertaken 1000 risks of £5000 each, at a fair premium of 1 per cent. per annum, so as to have an annual income of £50 000, to be expended in the payment of losses. It would be very inconvenient to such an office to be liable to the frequent occurrence of an annual loss amounting to twice its income; and in fact the chance of such an occurrence is only about 1 in 333. (1)

If the whole income of such a society were derived from 500 risks of £10 000 each, the chance of a similar loss would be about 1 in 32, (2) which is a degree of probability that ought not voluntarily to be incurred without some very powerful motives.

Still less would it be justifiable to engage the whole responsibility of such a society in 100 risks only of £50 000 each; the chance of losing at least £100 000 in a year becoming, in this case, somewhat more than 1 in 4. (3)

But it is equally demonstrable that a very exact adherence to the *precise amount* of the risk, which is thought most eligible, cannot be considered as essential to the reasonable security of the society. Thus, admitting the propriety of confining the risks in general to 1000 of £5000 each; if a few risks, not more than 10 for instance, of double the amount, were added to the number, the chance of losing an additional year's income, or £50 000, by the failure of 5 of them, would be no more than 1 in about five

millions (4): if 20 double risks were accepted, the chance of losing 5 would still be only 3 in two millions (5): and if the same number of single risks were rendered more hazardous, so as to require a double percentage; that is, if 20 risks of £5000 were accepted at 2 per cent., the chance of losing a year's income by these would be next to nothing, and that of losing £30 000 would be only 2 in a million. (6)

It may be observed, that the increase of the per-centage, on a risk which becomes more hazardous, on any given sum, brings with it an adequate remedy, in the increase of income, as well for the combinations of risks, as for the single adventure, without the necessity of any diminution of the amount of the separate risks.

Taking, for example, the extreme case of a risk of cent. per cent. on 10 adventures of £5000 each; it is *certain* that the *whole* income will be lost, but impossible that the loss should at all *exceed* the income. (7)

If the same income were derived from 10 adventures of £50 000 at ten per cent., there would be about one chance in three that nothing would be lost, and a little more than one in four that at least twice the income would be lost. (8)

With 100 risks of £5000 each, at ten per cent., the chances of losing twice the income are only 1 in 505; the danger of exceeding the limit being less than one hundredth part of the danger incurred from the same number of risks of £50 000 at 1 per cent., and little more than half as great as the danger with 1000 risks at one per cent. (9)

It may also be inferred, from the examples here computed, that when an office is well established, and is prudently conducted, there can be no practical necessity for having in readiness a deposit, exceeding the amount of the annual income, which is supposed to be £50 000; it is also obvious that a similar sum would be amply sufficient for all the contingencies that are at all likely to occur in such an office, from the time of its foundation, during the gradual accumulation of its transactions, to the supposed extent.

The calculations on which these assertions are founded, are subjoined at large in a Note.

The same mode of computation is equally applicable, whether the whole of the contingencies concerned are of the same, or of different kinds, provided that the risks be fairly estimated; and the result would by no means be affected by their dissimilarity. There is a common prejudice, that it is disadvantageous for an office to take a single risk of any particular description; and it is sometimes said, that if the adventure should happen to be unsuccessful, there would be no possible compensation from others of the *same* kind: there is, however, just the same chance that it would be compensated by others of a *different* kind; and if it were not, a fair price has been received for the responsibility, which it was worth while to incur from the probability of escaping, provided that the magnitude of the adventure was not too formidable; although the impression made *on the mind* by a singular event, is sometimes stronger and more disagreeable than by a more ordinary one. No person has ever attempted to assign, by calculation, what the precise disadvantage is that attends on a singularity of risk; nor has it ever been defined what degree of singularity there must be, in order to render a risk ineligible, except it involved a practical difficulty in appreciating it. It cannot be determined, for example, whether or no an insurance on the life of a negro, a mulatto, or an Indian, for an *adequate* premium, is a different *kind* of risk from an insurance on the life of a European; and if the affirmative were asserted, it would be difficult to show that a single insurance from a given town or county ought not to constitute an objectionable risk for a similar reason. And if it were possible to distinguish any imaginable diversity in the kinds of risks, so as to have, for example, 1000 kinds; and if 1000 equal risks of the separate kinds were at first undertaken by 1000 different offices, it is manifest that if each of these 1000 offices, instead of confining themselves to the same kinds of risks, exchanged, in the second place, one of its risks with each of the other 999, so as to have all heterogeneous, instead of all homogeneous risks, the profits and losses

of each office being supposed to be fairly balanced on the original supposition, they would remain equally balanced under the new distribution; nor is there any thing in the supposed change of combination that could affect the liability to greater or less deviations: at least, if there were any accident that could lead to such an inconvenience, it would probably be rather more likely to be diminished by the combination of heterogeneous elements, than by the confining the separate societies to their primitive homogeneous undertakings, which might possibly partake more of the nature of an undivided single risk, in the liability to inconvenient fluctuations.

A. B. C. D.

7, Waterloo Place, 24 Aug., 1826.

NOTE.

The number of n ary combinations out of m things being called $m^{(n)}$, we have $m^{(n)} = m \cdot \frac{m-1}{2} \cdot \frac{m-2}{2.3} \dots \frac{m-n+1}{2..n}$ and $m^{(n+1)} = m^{(n)} \cdot \frac{m-n}{n+1}$. And the probability of the joint occurrence of n events, of which the separate probability is p , in m trials, will be $m^{(n)} p^n (1-p)^{m-n}$, and for $n+1$ events this quantity must be multiplied by $\frac{m-n}{n+1} \cdot \frac{p}{1-p}$. In the cases which have been considered, we have (first) $m = 1000, p = .01$; (2) $m = 500, p = .01$; (3) $m = 100, p = .01$; (4) $m = 10, p = .01$; (5) $m = 20, p = .01$; (6) $m = 20, p = .02$; (7) $m = 10, p = 1$; (8) $m = 10, p = .1$; (9) $m = 100, p = .1$; and we must compute the value of the expression in each for $n = 0, 1, 2$, and so forth, as far as 10 or 20.

$$(1) \quad m = 1000, p = .01, 1 - p = .99, \frac{p}{1-p} = \frac{1}{99}.$$

Log. $1-p$	9.9956351946	Probability	of
$(1-p)^{1000}$	5.6351946	.00004317	0 loss
$\frac{p}{1-p}$	8.0043648		
1000	3.0000000		
	6.6395594	.00043607	1.

Practical Application of

		Probability	of
	6.6395594	.00047924	less than 2
999	2.9995655		
1 : 2	9.6989700		
1 : 99	8.0043648		
	7.3424597	.00220021	2
998	2.9991305		
1 : 3	9.5228787		
	8.0043648		
	7.8688337	.00739322	3
997	2.9986952		
1 : 4	9.3979400		
	8.0043648		
	8.2698337	.01861374	4
996	2.9982593		
1 : 5	9.3010300		
	8.0043648		
	8.5734878	.03745310	5
995	2.9978231		
1 : 6	9.2218487		
	8.0043648		
	8.7975244	.06273710	6
994	2.9973864		
1 : 7	9.1549020		
	8.0043648		
	8.9541776	.08998654	7
993	2.9969492		
1 : 8	9.0969100		
	8.0043648		
	9.0524016	.11282400	8
992	2.9965117		
1 : 9	9.0457575		
	8.0043648		
	9.0990357	.12561330	9
991	2.9960736		
1 : 10	9.0		
	8.0043648		
	9.0994741	.12574020	10
990			
1 : 11	8.9586073		
1 : 99			
	9.0580814	.11439260	11
		.69743325	

	9.0580814	Probability of	
		.69743325	less than 12
989	2.9951963		
1 : 12	8.9208187		
1 : 99	8.0043648		
	8.9784612	.09516148	12
988	2.9947569		
1 : 13	8.8860567		
	8.0043648		
	8.8636396	.07305325	13
987	2.9943171		
1 : 14	8.8538720		
	8.0043648		
	8.7161935	.05202279	14
986	2.9938769		
1 : 15	8.8239087		
	8.0043648		
	8.5383439	.03454171	15
985	2.9934362		
1 : 16	8.7958800		
	8.0043648		
	8.3320249	.02147953	16
984	2.9929951		
1 : 17	8.7695511		
	8.0043648		
	8.0989359	.01255844	17
983	2.9925535		
1 : 18	8.7447275		
	8.0043648		
	7.8405817	.00692757	18
982	2.9921115		
1 : 19	8.7212464		
	8.0043648		
	7.5583044	.00361663	19
		.99699465	under 20
		.00300535	20 or more.

(2) $m = 500, p = .01.$

Log. $1 - p$	9.9956351946	Probability of	
$(1 - p)^{500}$	7.8175973	.00657048	no loss.
500	2.6989700		
1 : 99	8.0043648		
	8.5209321	.03318426	1
		.03975474	

Practical Application of

	8.5209321	Probability of	
		.03975474	less than 2
499	2.6981005		
1 : 2	9.6989700		
1 : 99	8.0043648		
	8.9223674	.08363100	2
498	2.6972293		
1 : 3	9.5228787		
	8.0043648		
	9.1468402	.14022976	3
497	2.6963564		
1 : 4	9.3979400		
	8.0043648		
	9.2455014	.17599540	4
496	2.6954817		
1 : 5	9.3010300		
	8.0043648		
	9.2463779	.17635100	5
495	2.6946052		
1 : 6	9.2218487		
	8.0043648		
	9.1671966	.14695910	6
494	2.6937269		
1 : 7	9.1549020		
	8.0043648		
	9.0201903	.10475875	7
	2.6928469		
493	9.0969100		
1 : 8	8.0043648		
	8.8143120	.06520967	8
	2.6919651		
492	9.0457575		
1 : 9	8.0043648		
	8.5563994	.03600803	9
		.96889745	under 10
		.03110255	10 or more.
(3) $m = 100, p = .01.$			
Log. $1 - p$	9.995635195	Probability of	
$(1 - p)^{100}$	9.5635195	.3660330	no loss.
100	2.		
1 : 99	8.0043648		
	9.5678843	.3697297	1
		.7357627	under 2
		.2642373	2 or more.

(4) $m = 10, p = .01.$

Log. $1 - p$	9.995635195	Probability	of
$(1-p)^{10}$	9.9563519	.9043820	0
10	1.		
1 : 99	8.0043648		
	8.9607167	.0913517	1
$\frac{9}{2}$	.6532125		
1 : 99	8.0043648		
	7.6182940	.0041523	2
8	.9030900		
1 : 3	9.5228787		
	8.0043648		
	6.0486275	.0001118	3
$\frac{7}{4}$	.2430380		
	8.0043648		
	4.2960303	.0000020	4
		.9999998	

(5) $m = 20, p = .01.$

Log. $1 - p$	9.995635195	Probability	of
$(1-p)^{20}$	9.9127039	.8179070	no loss.
20	1.3010300		
1 : 99	8.0043648	.1652336	1
	9.2180987		
$\frac{19}{2}$	.9777236		
	8.0043648		
	8.2001871	.0158557	2
$\frac{18}{3}$	.7781513		
	8.0043648		
	6.9827032	.0009609	3
$\frac{17}{4}$	.6283889		
	8.0043648		
	5.6154569	.0000413	4
		.9999985	under 5
		.0000015	5 or more.

$$(6) \quad m = 20, p = .02, \frac{p}{1-p} = \frac{1}{49}.$$

Log. $1-p$	<u>9.991226076</u>	Probability of	
$(1-p)^{20}$	9.8245215	.6676080	no loss.
$\frac{p}{1-p}$	8.3098039		
$\frac{20}{20}$	<u>1.3010300</u>		
	9.4353554	.2724930	1
$\frac{19}{2}$	.9777236		
	<u>8.3098039</u>		
	8.7228829	.0528303	2
$\frac{18}{3}$	.7781513		
	<u>8.3098039</u>		
	7.8108381	.0064690	3
$\frac{17}{4}$	.6283889		
	<u>8.3098039</u>		
	6.7490309	.0005611	4
$\frac{16}{5}$	.5051500		
	<u>8.3098039</u>	.0000366	5
	5.5639848	.9999990	under 6

$$(8) \quad m = 10, p = .1 \frac{p}{1-p} = \frac{1}{9}.$$

Log. $1-p$	<u>9.95424251</u>	Probability of	
$(1-p)^{10}$	9.5424251	.3486784	no loss.
$1:9$	9.0457575		
$\frac{10}{10}$	<u>1.</u>		
	9.5881826	.3874205	1
		.7360989	under 2
		.2639011	2 or more.

(9) $m = 100, p = .1$			
Log. $1 - p$	9.954242509	Probability	of
$(1 - p)^{100}$	5.4242509	.00002656	no loss.
1 : 9	9.0457575		
100	2.		
	6.4700084	.00029513	1
99	1.9956352		
1 : 2	9.6989700		
	9.0457575		
	7.2103711	.00162312	2
98	1.9912261		
1 : 3	9.5228787		
	9.0457575		
	7.7702334	.00589160	3
97	1.9867717		
1 : 4	9.3979400		
	9.0457575		
	8.2007026	.01587460	4
96	1.9822712		
1 : 5	9.3010300		
	9.0457575		
	8.5297613	.03386580	5
95	1.9777236		
1 : 6	9.2218487		
	9.0457575		
	8.7750911	.05957870	6
94	1.9731278		
1 : 7	9.1549020		
	9.0457575		
	8.9488784	.08889520	7
93	1.9684829		
1 : 8	9.0969100		
	9.0457575		
	9.0600288	.11482300	8
92	1.9637878		
	9.0457575		
1 : 9	9.0457575		
	9.1153316	.13041621	9
91	1.9590414		
1 : 10	9.		
	9.0457575		
	9.1201305	.13186527	10
		.58315519	

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	9.1201305	Probability of	
	1.	.58315519	under 11
(90)	1.		
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	9.0787378	.11987750	11
89	1.9493900		
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	8.9947041	.09878797	12
88	1.9444827		
	8.8860566		
1 : 13	9.0457575		
	8.8710009	.07430207	13
87	1.9395193		
1 : 14	8.8538720		
	9.0457575		
	8.7101497	.05130380	14
86	1.9344985		
1 : 15	8.8239087		
	9.0457575		
	8.5143144	.03268258	15
85	1.9294189		
1 : 16	8.7958800		
	9.0457575		
	8.2853708	.01929170	16
84	1.9242793		
	8.7695511		
1 : 17	9.0457575		
	8.0249587	.01059153	17
83	1.9190781		
1 : 18	8.7447275		
	9.0457575		
	7.7345218	.00542652	18
82	1.9138139		
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ART. VIII. ANALYSIS OF SCIENTIFIC BOOKS.

- I. *Papers on Naval Architecture, and other Subjects connected with Naval Science; conducted by William Morgan and Augustin Creuze, Naval Architects, formerly Students at the School of Naval Architecture in his Majesty's Dockyard at Portsmouth. No. 1. To be continued half-yearly.* London: G. B. Whittaker, Ave-Maria Lane.

IF there is one subject which ought to interest us as a nation more than another, it is the efficiency of the civil and naval departments of our marine; and we do not hesitate to say, that one of the principal means of maintaining our present maritime superiority, is by paying the strictest attention to the theoretic construction, as well as the practical building, of our warlike and commercial navy.

It has been said, and with justice, that we generally excel foreign nations in the practical part of the profession; at the same time candour requires us to acknowledge, that some of the most advantageous improvements in our shipping have been borrowed from our neighbours. The present excellent system of diagonal framing is a modification of plans many years ago suggested in France; the introduction of shelf-pieces was derived from the same source. In making these observations, it is far from our intention to detract from the merit of Sir Robert Seppings. Although he is to be admired for adopting the enlightened views of foreigners, when those views are beneficial to his country, he may yet rest his fame on a more imperishable basis—his *own* improvements. His angular docking-blocks, thick waterways, forked knees, and binding-strakes; his made floors, short timber-frame, and ingenious masts, together with the general regularity introduced in the building and repairs of ships, combined with the care taken to ensure the free circulation of air, (which is the greatest preventive to premature decay,) are improvements in H. M. Navy which reflect honour on his talents and industry.

But although we may congratulate ourselves on the advancement made of late years in the *practice* of ship-building, it is a source of regret, that whilst other sciences have made rapid strides towards perfection in our highly-favoured land, the *theory* of Naval Architecture, which can yield to few in importance, to none in difficulty, has remained in obscurity. In further considering this interesting subject, it should be generally understood that ship-carpentry, and the design of ships, are totally different in their nature: an individual may excel in the former, and not even be acquainted with the rudiments of the latter. The same prin-

ciple is evident in Civil Architecture; the mind of the plodding character who might superintend the practical part of the building of St. Paul's, might be very incompetent to the majesty of the design.

In the present enlightened state of society, it seems scarcely necessary to dwell on the advantages which may be afforded to any art, or profession, from possessing the privileges of a liberal education; yet, singular as it may appear, there are those who maintain, that the construction of ships has received no assistance from science. To these we would observe, that "experiment used to be considered the road to knowledge:" evidence has commonly been considered a thing to be examined, and, if true, to be admitted. It is probable that opinions on this subject have altered since the days of Bacon*; if not, it can be easily proved, that in those countries where youth have been scientifically educated as naval engineers, where academical honours have been conferred on the meritorious; in fine, where government has extended its patronage, and conferred rank and consideration on professional officers, that *there* the best ships have been constructed. Not to mention numerous other examples, if we only direct our attention to the superior qualities of the Canopus†, the Rivoli, the Norge, and the Tonnant, we feel convinced that the acknowledged excellency of these ships will be a sufficient refutation of the assertion. Again, if foreign ships have not generally proved better sailers than British ships, how can we account for the eagerness expressed by some of our most experienced naval officers to obtain the command of them?

It is not much more than a century ago that the theory of naval architecture was in as degraded a state in France, as (till very lately) it was in England; for we are informed that "*La construction des vaisseaux étoit abandonnée à des simple charpentiers, et l'on ne pensoit pas que l'architecture navale fût fondée sur une application continuelle de la mécanique et de la géométrie, qui sont les branches les plus difficiles des mathématiques. Ceux qui exerçoient cette profession étoient seulement guidés par leurs lumières naturelles, et par leur propre expérience; ils variaient la forme des vaisseaux selon qu'il leur paroissoit convenable; ils se fondoient sur le recit des navigateurs, et en adoptaient très souvent les préjugés: flottant ainsi dans les espaces immenses de l'erreur, ce n'étoit que par un hasard singulier, qu'ils pouvoient parvenir à faire des vaisseaux qui eussent de bonnes qualités ‡.*" The French, however, have for some time removed the imputation

* Dr. Mac Culloch. Last No. of *Journal of Science and Arts*.

† The Ganges, Formidable, &c., one of the finest class of vessels in our service, were copied from the Canopus, which was taken from the French in 1798.

‡ Levêque's Preface to the translation of Don Juan's *Examen Maritimo*.

of neglect of marine architecture; they too well foresaw the advantages to be obtained from the encouragement of this science; and many a gallant commander of our fleets can bear sorrowful testimony to the escape of the enemy's ships, from their superior sailing qualities. The Swedes, the Danes, the Dutch, the French, have all cultivated theoretic ship-building with more or less success. Men of the greatest acquirements have devoted their attention to it: whilst Germany can boast of the efforts of Euler, whilst France may pride herself on the abilities of d'Alembert, Bouguer, &c., whilst Spain may justly cherish the memory of Don Juan, and Sweden of Chapman, Great Britain alone, who has possessed the almost exclusive sovereignty of the seas, has most unaccountably neglected the cultivation of this important science. Is it then a matter of surprise that naval architecture has advanced so slowly towards perfection in England? Ought we not rather to wonder that we have so many good ships? for which, by the by, we are more indebted to the valour of our navy, than to the judgment of our constructors; for the former capture ships, of which the latter make servile imitations; thus *publicly* confessing our want of skill, and the enemy's superiority, in a science of such vital interest to the empire.

One of the principal efforts to rouse the nation from the apathy into which it seemed to have fallen, was the establishment of a society for the improvement of naval architecture. This was a truly patriotic undertaking, and, from the support generally afforded to it by the public, and from the zeal, respectability, and well-known talents of several of its members, we might have expected more beneficial results from their labours. The committee consisted of twelve gentlemen, among whom may be noticed the distinguished names of Sir Joseph Banks, Dr. Hutton, and Professor Martyn. It is not our intention to enter into a detailed account of the proceedings of this society; the motives which actuated the members were to be admired; the almost complete failure of their patriotic intentions to be deplored.

It is not to be expected that a subject of such importance as the construction of the British navy, in which millions of the public money may be either uselessly expended, or judiciously saved, would be neglected by the legislature; it has occasionally excited parliamentary discussion. On the 7th of January, 1795, Mr. Robinson urged the necessity of attending to the construction of our ships of war, on the ground of their acknowledged inferiority to those of the French; when Admiral Gardner was of opinion that the superior construction of French ships arose from their government offering a premium for the best models. After a few observations from Mr. Fox, who blamed, in rather vague and general terms, the Admiralty, and indeed all the branches of the naval administration, Mr. Dundas (afterwards Lord Mel-

ville) rose, who very justly accounted for our inferiority, from the want of countenance and support afforded to shipwrights; for as projectors were not looked upon in Britain with much respect and confidence, they had little excitement to perseverance. After a few remarks from Mr. Sheridan, Mr. Pitt, &c., a college, with a board, was recommended for the improvement of naval architecture.

We now come to a period, in which considerable improvements were made in the civil departments of the navy, chiefly owing to the reports of the commissioners appointed by his late Majesty to revise the civil affairs of the navy. Among other important matters, their attention was particularly directed to the construction of our ships of war. After observing, that "whatever can be considered as relating to the construction of ships, is committed almost exclusively to the two surveyors, who are both members of the board," they add, "this very important part of the duties of the Navy-board being left so completely to the surveyors, it becomes natural to inquire by what means, or by what plan of education they are, according to the present system, prepared for a charge, in which the interest of the country is so deeply concerned." Accordingly, the second part of the third report is headed, "Present System of Education of Shipwrights," and commences by observing, "that apprentices are admitted at the age of fourteen; that, at their admission, many of them cannot read or write, few have much education; they are examined previously to their being entered, but their examination seems principally confined to their being of the age mentioned, to their being 4 feet 10 inches high, and found by the surgeon in sound health. As apprentices, they serve seven years; no care is taken to teach them anything during that time but their business as shipwrights, &c. &c." From the situation of quartermen "they are raised in succession to those of foremen, assistant to the master-shipwright, and to that of master-shipwright, from which employment the surveyors of the navy are chosen."

"In the whole course we have described, no opportunity will be found of acquiring even the common education given to men of their rank in life; and they rise to the complete direction of the construction of the ships, on which the safety of the empire depends, without any care or provision having been taken, on the part of the public, that they should have any instruction in mathematics, mechanics, or in the science or theory of naval architecture."

It is impossible for a lover of his country to read the following quotation from the same report, without feelings of humiliating regret: referring to the construction of our ships of war, they observe, "Where we have built exactly after the form of the best French ships that we have taken, thus adding our dexterity in building to their knowledge in theory, the ships, it is generally

allowed, have proved the best in our navy; but, whenever our builders have been so far misled, by their little attainments in the science of naval architecture, as to depart from the model before them in any material degree, and attempt improvements, the true principles on which ships ought to be constructed being imperfectly known to them, have been mistaken or counteracted, and the alterations, according to the information given to us, have in many cases done harm. From the same cause there has been infinite variety in the alterations made, and in the forms which have been adopted. The alterations being founded on no certain principles, no similarity in the form of the ships could be expected; and they have the appearance of having been constructed on the chance that, in the multitude of trials made, some one might be found of superior excellence. While therefore our rivals in naval power were employing men of the greatest talents, and most extensive acquirements, to call in the aid of science for improving the construction of ships, we have contented ourselves with groping on in the dark, in quest of such discoveries as chance might bring in our way. Nothing certainly can be more surprising, than that in a nation so enlightened as this is, and whose power, importance, and even safety, depend on its naval superiority, matters so essential to the preservation of that superiority should so long have been neglected."

No unprejudiced mind can regard the labours of these statesmen but with admiration. The luminous detail of all the particulars relating to the economy of our naval arsenals; the exposure of existing abuses, together with remedies for correcting them, show an intimate acquaintance with the subjects of their consideration, and great industry and perseverance in the compilation of their report. To remove the evils pointed out in the above extracts from their third report, the commissioners recommended the establishment of an institution for the purpose of improving the theoretic construction of the British navy. In accordance with their views, by the King's order in council, September 20th, 1809, a superior class of shipwrights was established at the dock-yard, Portsmouth. The commissioners of naval revision were above that narrow-minded policy which would sacrifice the ultimate good of the public to the present individual interest of a party. They accordingly planned the above institution on the most liberal principles, no other eligibility being required for admittance than the candidates' being subjects of Great Britain. They are examined, previous to admission, before the commissioners of the dock-yard, the professor of the Royal Naval College, and the lieutenant-governor. They are required to be intimately acquainted with the English language, so as to write it grammatically, and from dictation; they are to be able to read and translate the French language; they must be well grounded in the

first six books of Euclid, together with the eleventh, and algebra as far as quadratic equations. A printed paper is placed before each candidate, with a number of geometrical and algebraical problems, which he works out on paper; and those are selected as the successful candidates who have displayed the greatest talent in the examination. They remain in this establishment seven years; during which time they go through a course of mathematics, including geometry, algebra, trigonometry, mechanics, hydrostatics, and fluxions, which is now substituted by the differential and integral calculus. They are instructed in the theory of naval architecture, by studying the stability of floating bodies, and some of the best works in the French language on ship-building. After obtaining sufficient elementary knowledge, they are employed in constructing original designs of ships of various classes; in ascertaining their displacements, the centers of gravity of the displacement, the centre of gravity of the whole ship, and equipment, considered as an heterogeneous body; in calculating their stabilities, both according to the French method, from the height of the metacentre, and likewise according to the more correct plan recommended by Atwood: in fact, by a judicious comparison between the qualities of English and foreign ships, they endeavour so to combine the good qualities, and remedy the bad, as to improve the general design.

But desirable as these studies are for a naval architect, an accurate knowledge of the practical part of the profession is not less indispensable; accordingly, the chief portion of their time is occupied in laying off ships in their practical construction, and in making the drawings which are necessary for the execution of the work in the progress of building. An officer is particularly appropriated to this important part of their duty. They are annually examined before the commissioners of the Dock Yard, the admiral of the Port, the lieutenant-governor, and the governor of the Royal Naval College, and School of Naval Architecture, who is the first lord of the Admiralty. These examinations both in mathematics and ship-building, are very severe, and considerable study and preparation are required in order to pass them creditably. After finishing their course of studies at the School of Naval Architecture, they are removed to the different dock-yards, and fill the situations of subordinate officers: from which situations it is the professed intention of government to promote them to those of the higher offices, and eventually to that of surveyor of the navy.

We have now given a general outline of this institution, and we need scarcely add, that it is capable, with proper encouragement, of being very beneficial to the country.

Every measure of government, however enlightened, will meet with opposition from some source or other, accordingly this has

found its opponents, chiefly among those who affect to believe that scientific attainments are incompatible with practical experience. If by experience is meant a continuance in error arising from the prejudices of custom, we allow the position to be correct: but if by practical experience is meant the best adaptation of the materials of which a ship is composed, to the purposes for which they are designed; or the most judicious combination of the materials of the fabric, so that with a *minimum* of expense we may obtain a *maximum* of strength: if by experience is meant those general, correct, and philosophical deductions derived from a just comparison between different experiments, under different circumstances; we answer that science alone can produce these effects in an eminent degree. That individual is the best practical man, and will ultimately prove the most useful servant of government, whose practical determinations in the execution of his duty, flow, as it were, unconsciously from his mind, although these determinations are, in fact, the result of patient study and scientific investigation. Who would imagine that the practical knowledge of Rennie, of Watt, of Smeaton, or of Wren, would have been greater; or that their aptitude for business and for carrying on their extensive duties, would have been increased, if they had been unacquainted with the scientific principles which guided them? Or what reason have we to suppose that the practical knowledge of Du Hamel, and of Chapman, would have been increased, had they been ignorant of the principles of geometry, or the application of mechanics to the science of naval architecture. We may rest assured that accuracy in accounts, economy in expenditure, ability in correspondence, and energy in the general routine of business, will be best ensured when the principals of any department possess those enlarged views which naturally result from the cultivation of the mind.

It has been further asserted, that although this Institution has been founded sixteen or seventeen years, although some of the senior members are between thirty and forty years of age, that there is no demonstrative proof before the public, of improvement in this science. Before we admit the justice of a decision, we should be fully convinced of the competency and impartiality of the judge. Now it is impossible for persons who are utterly ignorant of mathematical principles, to duly appreciate laborious calculations on the properties of ships. They little think of the time and trouble required for calculating one of the elements, the centre of gravity. If they are informed, that to obtain this point (independent of experiment), you must multiply the weight of every portion of the ship's stores, ballast, provisions, &c., by its distance from a certain plane, and divide these momenta by the whole weight of the ship and lading, they form but a vague idea of the immensity of the undertaking. The secret workings of intellect, if slow in their operations, will not be the less certain in

their results. The advancement of a new institution is necessarily slow; many years must elapse before its full effects are evident to the country: time is required for obtaining knowledge, time is required for directing this knowledge to the most beneficial operations; above all, time is required to remove that greatest bane to improvement, deeply-rooted prejudice. They as yet fill but subordinate situations in our dock-yards, consequently they have neither influence nor authority to forward the views of their enlightened patrons. But we may confidently affirm, that if much remain to be done, much has been done. Before the existence of this establishment, the centre of gravity of a British ship was never ascertained: there now exists an analysis (at present very limited) of some of the best ships of our navy. This institution has spread a spirit of inquiry in the nation, and the chief that is known in this country on the theory of naval architecture, may be traced either directly or indirectly to it. Independent of these considerations, let the fact that the only scientific work entirely devoted to naval architecture *, ever published in great Britain, (except translations,) is conducted by two members of this Institution, Messrs. Morgan and Creuze, bear honourable testimony to its usefulness. Let the work stand on its own merits, and if, from an impartial examination of its contents, we find that anything is added to our present store of knowledge, if any fresh truths are elicited, or old truths presented in a more simple or judicious form, we candidly confess that the expense government has incurred in their education, will be more than counterbalanced by the ultimate advantages which will accrue to the country from their exertions.

The first number of this periodical is dedicated to the Rev. Dr. Inman, who, in conjunction with his duties at the Royal Naval College, has also the direction of the studies of the students at the School of Naval Architecture. His learning and decision of character, combined with affectionate zeal for the interest of both establishments, have gained him the deserved respect of those who have been educated under him: the following extract from the dedication is a just tribute to his merits. "It is not too much to say, that not only much of the present knowledge of the theory of naval architecture in this country, but the probability of improvement in this important science, is the effect of that enlightened and decided conduct by which you have been enabled to carry into execution the views of the Right Honourable Board of Admiralty, by the application of mathematical science to this subject, in the instruction of the students educated at the School of Naval Architecture for officers educated in His Majesty's dock-yards," and indeed the direction of their duties, the works he has produced for their instruction, and his translation, and valuable

* We of course except the valuable papers of Atwood in the *Phil. Trans.*, and the articles by Professor Robison in the *Encyc. Brit.*

notes to the celebrated treatise of Admiral Chapman on Naval Architecture, show that the Right Honourable Board have not misplaced the confidence they have reposed in him.

The introductory remarks on the state of naval architecture will be read with interest by all classes. The error of supposing that our ships need no alteration because we have been generally successful against the enemy, is fully exposed. We have already alluded to this, but the following observations are so much to the purpose, that we cannot but quote them. "This consideration may have arisen from not making a proper distinction between the qualities of our ships and the characters of our sailors. The extraordinary and brilliant success of our navy during the last war, was owing altogether to the ability of our naval officers, and the intrepidity of British seamen. To the superiority of our ships, this success can never with propriety be attributed, as was fully confirmed by the accounts given of their qualities, by many officers of distinguished merit. The question appears rather to be, whether their defects did not, in some instances, render the courage and abilities of our sailors unavailing."

In alluding to our copying ships of foreign nations, instead of designing for ourselves, it is observed, "Instances, however, not unfrequently occurred, in which ships built agreeably to the forms of these models, did not fully answer the expectation raised of them. This might proceed, in some cases, from altering the size proportionally, so as to build ships of similar, but not equal, dimensions." This was illustrated in a class of vessels, the *Bann*, the *Cyrus*, the *Hind*, and several others, which were built on the reduced lines of the *Bonne Citoyenne*. This ship was taken from the French, and was distinguished for superior qualities, although the ships which were built similar to her, but on a smaller scale, were very inferior to her. On this subject we may obtain considerable information from Chapman's reasoning. He proves that if *L*, *B*, *D*, respectively represent the length, breadth, and depth of a ship, the velocity, within very inconsiderable limits, will be

proportional to $\frac{B^{\frac{1}{2}} \times L^{\frac{4}{3}}}{D^{\frac{3}{2}}}$. "From the above expression for the

velocity, we see that a large ship should sail better than a small one, which is similar to it, but that nothing farther is required than to diminish the draught of water, to render the small one as good a sailer as the great one. For if the velocity of the large one

$$= \frac{B^{\frac{1}{2}} L^{\frac{4}{3}}}{D^{\frac{3}{2}}} = 12; \text{ if the length, breadth, and draught of water}$$

of the small one be, *l*, *b*, *d*, the depth *d* must = $\left(\frac{b^{\frac{1}{2}} \cdot l^{\frac{4}{3}}}{12} \right)^{\frac{2}{3}}$ in

order that the two ships, built of the same form, may sail equally well *." But not only the alteration of the size might prevent ships sailing equally well as the original, but, the introductory remarks continue, "It might generally arise from not being acquainted with the detail of the circumstances connected with the design, and the dependence of the different properties on each other in producing the general good effect; so that by the alteration of some of the elements of the construction, either some of the dimensions, the quantity or specific gravity of the materials, or the arrangements of some of the weights, the excellency of the design, to say the least, might be greatly diminished." These observations show the inconsistency of building two ships from the same design, the one of oak, the other of fir; for if the oak ship possess every desirable quality, with a certain draught of water, it is very improbable that the fir ship will possess equal qualities, with considerably less draught of water; and even allowing that the fir ship would be materially improved by bringing her to the same draught of water as the oak ship by extra ballast, it is, to say the least, a very unscientific method of proceeding, independent of the expense that might have been saved by using less timber, if the original design had been altered. The same train of reasoning accounts for the inferiority of some ships, which, before we took them from the enemy, were remarkable for fast sailing; we may instance the American schooners. These vessels appear to have been built expressly for velocity: accommodation, and convenience of stowage, with even the necessary comforts for the officers and crew, seem to be secondary qualifications. They are put together in the slightest manner, with scarcely a divisional bulkhead for the separation of the stores. Their warfare is a species of privateering, and they only take to sea a few weeks' provisions. When they are fitted for our service, it is necessary, from difference of discipline, &c., to alter the internal arrangements of the vessel. She is divided into different compartments for the officers, and store-rooms for the stores. She may probably be considered weak, and her topside is strengthened with additional timber: all this tends to increase her rigidity, and render her less elastic: and the quantity of provisions, water, stores, and ballast, being also increased; her centre of gravity, stability, and all her properties, are in consequence altered. The particular service for which a ship was designed, ought to be one consideration, but not the only one, which should influence us in copying a foreign vessel. She ought not to be too closely imitated till we know that no *local* circumstances limited the original constructor in her design. Thus some nations would prefer giving their line-of-battle-ships more draught of water, but are prevented, from the shallowness of their harbours.

* Professor Inman's translation of Chapman's work.

The writer of this paper next refers to the works which have been published in England on this subject. Theoretical works have been very rare. "Atwood's papers in the *Philosophical Transactions* of 1796 and 1798, on the stability of floating bodies, with its application to ships, are almost the only theoretical works written in English on the subject." We are indeed under considerable obligations to Atwood, and it affords us gratification to reflect that an Englishman has, in any degree, advanced this science; his theory of the stability of ships is perfect: before his time the height of the metacentre was taken as a measure of the stability, which he proved to be only true when the angles of the inclination are infinitely small. "Several works on the practical part of the subject have been written, but none which can be properly characterized as scientific. The works of Sunderland, Mungo Murray, Stalkart, Steel, Fincham, and the *Shipwrights' Repository*, deserve the most attention; those of Steel are by far the most useful to a practical builder." England is indeed most singularly deficient in works of scientific eminence. On examining those above named, we shall find them generally to consist of methods of delineating the plans and inboard works of our ships, with rules for conducting the practical part of ship-building, tables of scantling, with imperfect methods of laying off, sufficiently near as approximations, but often very deficient in geometrical precision. The portions of these works which are devoted to science, are almost wholly brief extracts, and imperfect translations, of the French books. To the names of the above-mentioned should have been added that of Knowles, who has published an Appendix to the expensive work of Steel. Messrs. Knowles and Fincham are the only authors who have brought down the practice of ship-building to the present time. Mr. Knowles' Appendix is a valuable acquisition; his tables are particularly useful. Mr. Fincham's outline of ship-building being only designed for the elementary instruction of the students at the Royal Naval College, and School of Naval Architecture, he has not taken a very scientific view of the subject.

The necessity of these works is obvious; for so complete a change has taken place within these last twenty years, that practical directions for building ships before that time are of very little use. The whole combination of the fabric of a ship is altered. The scarphing of the timbers, the fastening of the beams to the side, the fitting of the sterns, the laying of the decks, in fact the whole disposition, has undergone a thorough revision, and every one must see the necessity of an elaborate work on the improvements which have been lately introduced in the civil departments of his Majesty's navy by Sir Robert Seppings and others. This work should also contain a full

account of the manufactures of our naval arsenals; rope-making, sail-making, anchor-making, rolling the copper, our unrivalled block-machinery, should be all explained in separate articles, subject to such restrictive regulations as the government should think proper. The portion of this volume, or rather volumes, which relate to the practice of naval architecture, should give a full account of all the later introductions, with practical rules for carrying on the duty. The merits of the diagonal framing should be explained, and all its advantages or disadvantages candidly discussed. In prosecuting this part of the work, it should first be shown what strength the timbers of the frame afford the ship; secondly, how the combination is strengthened by the external planking; lastly, what additional support is given to the fabric, both transversely and longitudinally, when the riders and trusses in the hold are introduced. A very interesting paper may also be written on the circular sterns of our ships of war. On a subject of such national consequence, the public should be fully and impartially informed. There can be no question, that, if many individuals are misled by prejudice and opposition against this introduction, that there are others, (particularly among our naval officers,) alike distinguished for integrity and patriotism, who conscientiously differ from Sir Robert in opinion. In this investigation all attempts at evasion should be studiously avoided: if the circular sterns are more expensive than the square sterns, let the point be yielded; if more heavy, concede this also, and let their merit stand on the grounds of their *strength, safety, and efficiency*: if these qualities can be proved (of which there can be little doubt), they are sufficient grounds for their general adoption; and to the man who would risk the efficiency of our ships, and consequently the honour of the British flag, for the sake of a few pounds, we yield all the credit he desires for his rigid economy, but do not envy him his feelings of patriotism.

The name of Mackonochie is also omitted in the list of English authors. He published a Prospectus of a work entitled "A Philosophical and Experimental Inquiry into the Laws of Resistance of Non-elastic Fluids and Cohesion of Fibrous Solids, as far as either is connected with the Theory or Practice of Naval Architecture;" also "Political and Commercial Strictures on the Comparative State of Naval Architecture in Great Britain and India," in two volumes, quarto. This Prospectus displays a mind of no common capacity; with an intellect capable of entering into the most complicated difficulties of the *theory*, he possessed the clearest conviction of our deficiency in the *practice*, and saw the immense advantages which would accrue to ships from a more scientific adjustment of the materials, so that strength might be produced rather by altering the disposition of

the materials, than "by augmenting their bulk, thereby filling indeed the spectator's eye, and conveying to him an appearance of strength, while to the mechanician, who considers its relative connexion, it presents only a ponderous mass of imbecility*." He endeavoured to obtain a body of well-arranged and useful facts, "for the advancement of an art, which, from its importance to the British empire, would be thought by a stranger the first object of national patronage, instead of being utterly abandoned to the desultory researches of individuals who may be directed by inclination or interest to make it the object of their attention†." In his "Inquiry into the Mechanical Structure of Fibrous Solids," he thus expresses himself: "It will be received with surprise, perhaps with contempt, by those who have been in the habit of viewing a ship as the noblest effort of the human genius, to hear it asserted, that in a mechanical point of view, (not nautically, politically, or commercially,) but merely as a machine composed of wood and iron, it is the feeblest, most inartificial, and unworkmanlike structure, in the whole range of mechanics‡." In illustrating this bold assertion, he shows, that the combination of the timbers, plank, beams, &c., of a ship, afford each other but little support, but are merely kept together by the fastenings; so that if it were possible to annihilate these fastenings, the whole fabric would become an undistinguished mass of ruins.

We cannot close our remarks on the introductory paper without recommending the observations in page 6, to the careful perusal of the naval profession. The following extract well deserves attention: "There is, however, another method of extending our knowledge of naval architecture, which may eventually lead to the most important results; the connexion of the experience of naval officers with the scientific knowledge of the naval architect. Every cruize might in a greater or less degree be an experiment; and the enumeration of the good and bad qualities of a ship may be the means of increasing the former, and reducing the latter in future constructions, &c."

The three following articles are on the displacement, the centre of gravity, and the stowage of a ship. Having the draught of a ship, the displacement is easily obtained by measuring the solid content of the part displaced, which is equal to the weight of the ship. The content of any irregular solid, or the area of any surface, may be found by Sterling's Rules, pub-

* From p. 37 of the Prospectus.

† From p. 22 of the Prospectus.

‡ From p. 27 of the Prospectus. It is unfortunate for the cause of science that Mackonochie's work never succeeded his prospectus. If his manuscripts be in existence, how desirable would it be to rescue them from unmerited oblivion.

lished in his *Methodus Differentialis*. These rules are explained, as also the methods of applying them, but not in so familiar a manner as to enable a person who had little or no previous acquaintance with mathematics to make use of them. The following valuable results of calculations were made according to the above rules:—

“ The displacement of the Caledonia, of 120 guns = 4803.3 tons
 Bulwark 76 = 3144.8
 Endymion 50 = 1695.0
 Icarus 10 = 278.8

The tonnage here obtained is the absolute weight of the ship, with all her stores, men, and provisions, when ready for sea. We will now give the tonnage of these ships, as inserted in our Navy Lists, which neither expresses the *displacement* of the ship, nor the *weight* she can take on board : we find that the

Caledonia = 2616 tons.
 Bulwark = 1940
 Endymion = 1277
 Icarus = 234

The rule for obtaining this tonnage is this: multiply the length of the ship by half the square of the breadth, and divide by 94. What the number 94 has to do with the capacity of a ship, would be difficult to determine. Evasion of the duties is not the least evil resulting from this rule; it is a check on the improvement of our merchant-shipping, by sacrificing expedition, and even safety, to burthen. Every one who considers this subject, is impressed with the necessity of some legislative measures to remedy the evil.

There is another piece of useful information given in this paper. “ The displacement of one inch in depth at the load-water section of the

Caledonia, of 120 guns, is 23.9 tons
 Bulwark 76 18.3
 Endymion 50 13.4
 Icarus 10 4.2

By consulting this little table, an officer may accurately estimate the difference of draught of water which will be produced, by taking out or putting in various weights.

The following results of calculations in Art. 3, “ on determining the centre of gravity of a ship,” will be duly appreciated. The height of the centre of gravity of the

Bulwark, of 76 guns, was .7 feet above the load-water section
 Ajax 74 .6 ” ” ”

These heights of these centres of gravity were found by the laborious calculations before-mentioned. The moment of every weight in the ship was obtained from a horizontal plane, and the

sum of these momenta was divided by the total weight of the ship.

But a much more easy method of ascertaining the centre of gravity is explained in p. 15, which might be practically adopted by the officers in our navy. The chief difficulty they would find is, after they had made the experiment, that they obtained the height of the centre of gravity above an *unknown* point, *viz.*, the centre of gravity of the displacement, so that if they cannot calculate this from the plan of their ship, they must be dependent on others to furnish them with it.

The 4th Art. on the "Stowage of Ships," is comprehensive, and the subject is considered with considerable ability, under the following heads: stability, rolling, pitching, holding a steady course, ardency or tendency to fly up in the wind, going about, action of the rudder, and the strain of the materials. This paper deserves the particular attention of our *young* naval officers: indeed, a short essay on the stability of ships, or their stowage, the centre of gravity, and all the most important properties, drawn up in a familiar manner, and divested as much as possible of abstract mathematics, would be of the most essential service to them. The hold of a ship is frequently broken up and restored, which is attended with great labour and trouble; sometimes with no good effect: now if our young officers were acquainted with the scientific principles which should influence these alterations, they would form a more correct opinion of the effect likely to be produced, instead of being indebted to the good or bad success of the experiment for judging of its merit.

The observations of an intelligent naval officer on the properties of his ship are invaluable, particularly on the stowage. If we examine the different reports of the sailing qualities of the same ship, when commanded by different captains, we see anomalies which at least may partly be accounted for from their difference of opinion on stowage. One officer makes his ship sail very much faster than another: would not the following plan tend to remedy this? After the best sailing trim of the vessel is ascertained, let the master make an accurate plan of the hold, with the situation, as well as the quantity of the ballast and stores of every description, distinctly delineated, also stating the draught of water of the ship. Let a copy of this document be given to the next master and officers who join her, and if, after various experiments, they find they cannot improve her velocity, they can, at all events (as far as stowage is concerned), ensure her sailing equally well as she did formerly.

But not only is it desirable to have comparisons of the same ship at different times, but also of different ships, when not sailing together. This may be effected by supplying each ship with an anemometer: perhaps the one recommended by Bouguer

would be the most simple. It consists of a square board, secured to a slight piece of wood or metal placed in its centre, and standing at right angles to its plane. This piece of wood or metal fits into a small cylinder composed of wood or metal, its lower end resting against a spring placed at the bottom of the cylinder. To graduate this instrument, you respectively place 1, 2, 3, &c. lbs. upon the board, and make an index on the piece at right angles to the board, as the pressure increases, and forces it down the cylinder. This instrument is very simple, and in case the force of the spring varies, it can be easily readjusted. The method of using it requires but little explanation. When an officer is going to register the velocity of his ship, let him place the plane of the anemometer perpendicular to the direction of the wind, and the index will show the force of the wind to be equal to so many lbs. on a square foot. He may then collect the following information: the draught of water of the ship; the exact distribution of the weights; the force of the wind on a square foot of canvass; the quantity of sail set; the angle to which the yards are braced; the number of degrees the ship inclines; and her velocity when under these circumstances. These elements, when embodied with those contained in the printed forms of the sailing qualities, furnished to the navy, would be a mass of information which could not fail of producing essential service. If these observations were taken by the midshipmen, and daily inserted in their log, it would excite an emulation in their minds, which would prove a powerful stimulus to them for obtaining more philosophical ideas of the properties of the ships they are destined to command. Above all, if the observations we have just alluded to were inserted in a tabular form, in the list of the navy (drawn up for the information of the Admiralty and Navy Boards), together with the light displacement, load displacement, area of midship section, area of load water-line, height of the centre of gravity of displacement, height of the centre of gravity of the ship, when equipped, respective distances of these centres before the middle of the length, height of the centre of effort of the wind on the sails, the stability at 5, 10, and 15 degrees of inclination, we should possess an analysis of our navy, which would form one of the most splendid collections of nautical information that any nation or country could boast of. This is an Herculean task, and would require the united talents and exertions of many individuals, but would amply repay the country for the expense incurred. Not only would it materially assist naval architects in future designs, but the Admiralty and Navy Boards would have a comprehensive view of the properties of our ships, placed in a simple form before them, and their lordships would judge better of the respective merits of the different ships they order to be

built. If this be ever put into execution, it must be undertaken by those whose whole energies are concentrated in their profession, and whose minds would rise superior to the difficulties they would encounter in their arduous task. They must be content to have their exertions undervalued by some, who would overlook what *has* been done, by referring to what *ought* to have been done; ridiculed by others, who, instead of having the "expectations of ignorance" realized by a *voluminous* account, would merely see the labours of years, the fruits of anxious days, perhaps sleepless nights, collected together in a few tables, condensed in a small space.

The next article in this work contains an abridged translation of the theory of the resistance of fluids, of Don George Juan, introduced by some general observations, which, although short, show a knowledge of the subject, which explain the intention of the conductors of this publication, to give "the different theories, and best courses of experiments in the successive numbers, and to make such inductions as may appear, from a careful examination of the whole, to be legitimate."

The abilities of this illustrious Spaniard (Don George Juan) are well known, not only as a naval architect, but also as a profound mathematician and philosopher. His theory differs from many others, from taking *depth* into his investigation. The value of this translation is evident; for so little was his theory known in England, that Professor Robison, in his article *Resistance*, Ency. Brit., observes, that he was unable to procure a copy of the original, and was, therefore, obliged to depend on the limited notice given of it by Prony.

The general reasoning in Art. VII., on the raking of ships' masts, is replete with sound sense and accurate observation, and it will afford pleasure to the intelligent naval officer to find the result of his experience confirmed by scientific reasoning.

The account of the experimental cruises of H. M. corvettes, Orestes, Pylades, and Champion, is drawn up with great impartiality and judgment. The author of this paper has entered very circumstantially into the particulars of each day's sailing, and nothing is wanting to enable the reader to judge clearly of the respective qualities of these vessels, but their draughts. It is generally known that the Orestes was designed by Professor Inman, the Pylades by Sir Robert Seppings, and the Champion by Captain Hayes.

The chief peculiarities in the forms of these vessels are, that the Orestes is full bowed, with a fine run, and a hollow from the keel to the floor-head.

That, either from a singular coincidence, or respect to the professor's judgment, Capt. Hayes gave his ship the *same dimensions*, and the same midship section, as far below water as the

stability is concerned; but that the chief difference between the *Orestes* and the *Champion* is, that the *Champion* is particularly full aft.

That the *Pylades* essentially differs from both these ships, as she has a straight rising floor, and partakes of the form of some of the fast-sailing French privateers.

When referring to the fulness of the after-body of the *Champion*, the writer of this paper judiciously observes, "but the peculiarity in the *Champion's* construction materially affected the *permanence* of her stability: she required her ballast to be stowed aft, consequently, its centre of gravity was much higher than it otherwise would have been; and, as the consumable parts of the stores were diminished, this had a proportionally greater effect in raising the centre of gravity of the system, and, therefore, diminishing the stability."

From a concise summary of the sailing qualities of these ships, during the three cruises, the writer shows, that their comparative merits will stand thus: *Orestes*, 58; *Pylades*, 51; and *Champion*, 49.

To the casual observer, these cruises will only be regarded as subjects of speculation or idle curiosity; but to the scientific inquirer, they afford so many valuable experiments to direct him in future designs. The different properties of these ships, under different circumstances, prove that the theory of naval architecture is not founded on chance, but on the immutable laws of nature, fixed in their principles, though often difficult in their application, and, therefore, requiring more talent and industry to unfathom them.

The liberal manner in which these trials were conducted, shows that our naval administration are determined to promote the interests of the country, by encouraging naval architecture; and no person who is anxious for the advancement of this science, but will feel satisfaction at the enlightened views of the Admiralty in directing these experiments.

We might easily extend our observations on the other papers in this interesting number, but our limits forbid. The manner in which this work is got up is respectable, and its contents are highly creditable to the professional abilities of the authors; and we confidently trust, that, in the progress of the "*Papers on Naval Architecture*," the national stigma, which has always been cast upon us, of being ignorant of this science, will be ultimately removed.

We possess advantages in England which no other country possesses. Independent of the experience we ought to have gained, from the number of our ships employed, there exists in the Navy-office drawings of all the ships taken from the enemy during many ages. This repository of knowledge, which contains the opinions of every maritime nation in the world, is a mine,

which, if we were aware of its riches, would not remain long unexplored.

Let not our boasted excellence in *practice*, lull us into contempt of *theory*: a well-built ship often proves a very bad sailer. Nothing is more prejudicial to the advancement of science than resting satisfied with our present knowledge. Peace is the time for improvement, which is fully exemplified by our continental neighbours, who are prying with eager inquisitiveness into the cause of our past success. Men of the greatest intelligence are sent by the respective European governments to visit our naval arsenals, and collect all the information in their power for the good of their country. Our Trans-atlantic friends are not less on the alert to improve their ships. Dupin is rousing all Europe to a competition with us in naval efficiency, and strenuously insisting on the propriety of re-establishing the ancient academy of marine, for the education of the French naval officers and engineers. Whilst we see all this, it surely becomes us to be on the alert, and cultivate a science which, as it is of such vital importance to us as a nation, should be our pride and our boast.

II. *Mémorial de l'Artillerie, ou Recueil de Mémoires, Expériences, Observations, et Procédés, relatifs au Service de l'Artillerie; rédigé par les soins du Comité Central de l'Artillerie de France, avec l'approbation du Ministre de la Guerre. No. I. Paris, 1826. (Ne se vend pas, et paroîtra à des époques indéterminées.)*

THIS is an octavo work of 320 pages,—undertaken by the order, and printed at the expense, of the French Government,—drawn up and edited by the Committee of the Central Depot of the Artillery at Paris; a committee consisting of seven general officers of Artillery: and is the first number of a periodical series, of which the successive numbers are to appear at intervals (it is understood) of one or two years. The impression is limited to 1500 copies, of which one is distributed by the Government to each of the officers of the Artillery, amounting to nearly 1200; and the remaining copies, according to a list of names inscribed by the Central Committee, and which must previously have received the sanction of the Minister at War.

The work which is thus designed to appear periodically, and to be thus distributed, is one of several measures which the Government of France has recently adopted, to stimulate the application, and to increase the professional acquirements of its officers of Artillery; to acquaint them with all that has been done, or is doing, in their own and in other countries, useful or interesting to an Artillery officer; to excite their emulation in studying, and to lead

them to suggest improvements, in the construction, or in the employment, of the various engines and apparatus of war which appertain to the service of the artillery.

We think that we may perform an acceptable service to our readers, in availing ourselves of the appearance of the first number of the *Mémorial*, printed in the present year, to acquaint them with the outline of (a part at least of) the measures which the French Government has adopted for this purpose; and with the advantages which, even in this early stage, appear to have resulted from them, in the zeal, activity, and intelligence, with which they have been met by the officers.

The *Mémorial de l'Artillerie* originated in an instruction communicated on the 10th of July, 1824, by the Baron de Damas, then Minister at War, to Lieutenant-General Comte Vallée, Inspecteur-Général du Service Central de l'Artillerie, directing the preparation by the Central Committee at Paris, and the printing at the expense of the Government for limited distribution, of a periodical work, which should have for its object to convey to the officers of the Artillery generally a knowledge of the successive improvements introduced, or the modifications adopted, in the *materiel*, or in the different branches of the service of the Artillery,—of experiments made or commenced, and of others proposed as deserving of consideration previously to their being made, having for their object the improvement of what may be specially termed the *science* of Artillery, or the correction or simplification of any of its practical details,—to be a record of inventions already existing in the Repositories, or which may hereafter be deemed worthy of admission there; giving of each a sufficiently precise description, and noticing the advantages or disadvantages which may have appeared on trial,—together with such extracts and translations from foreign works,—as also all such other matters, as, in the judgment and discretion of the Committee, should merit insertion, with a view to the propagation of valuable knowledge in the service.

The same communication from the Baron de Damas announced the intention of the Government to allot a sum of 3500 francs annually, to form three prizes, to be entitled “premiums of encouragement,” and to be given in recompense to officers, who in the course of the year shall have presented views or discoveries useful in one of the branches of the service of the Artillery; or who shall have resolved with success the questions which the Central Committee, viewing the points of the service to which it is most desirable that the attention of the officers should be directed, shall have proposed as subjects for their consideration: the questions to be lithographed and distributed to the officers: the success with which they may be resolved, to be judged of by the Committee, who are authorized to avail them-

selves in special cases of other judgments in aid of their own, but to be themselves responsible for the decision. The sum to be divided into three premiums—the first, of 1500 francs, and each of the other two of 1000 francs ; and each premium to be further liable to a division between two competitors, in cases where there is no decided superiority. There may, therefore, be as many as six premiums in one year, when each will be about the value of 600 francs ; or there may be fewer, when one or more will be of 1000 or 1500 francs value ; or finally, the premiums, or a part of them, may not be adjudged at all. It is further provided, that communications be distinguished by a private mark, such as a motto, or a sentence quoted from some work, which shall also be attached to a sealed paper containing the name of the writer, not to be opened until after the decision, and then only those which belong to the successful communications ; the papers containing the other names to be destroyed unopened. The premium to be presented in a medal, of the value of about 600 francs, and the difference of value (as when the premiums are not divided) to be given in money. The approved communications to have the further distinction of being inserted in the *Mémorial*, with the name of the author ; excepting when the subject is of a nature, in the view of the Government, to render publication objectionable.

The questions proposed for solution in the first year of the establishment of the premiums (1825) were the following ; viz.—

“ 1st. To contrive an instrument, or a system of instruments, and to describe a mode of procedure, by which the coincidence and the rectilinearity of the axes of the interior and exterior surfaces of cannon may be strictly verified.

“ 2d. What are the different cases in war in which Ricochet fire ought to be employed in preference to Direct fire ? What are the circumstances in which the Ricochet fire at high angles, and the same fire at low angles, ought respectively to be preferred ? If sufficient experiments are wanting, from whence the solution of this question may be derived, to propose the scheme of such experiments as may conduct to a solution. To examine specially the state of instruction of the French Artillery relative to Ricochet fire, and to point out the most simple means of its improvement.

“ 3d. The third subject is left in the first year to the discretion of the officers : all memoirs, which present new views, discoveries, or useful propositions, relative to any one branch of the art of the Artillerist, will be received.

“ The communications to be completed and sent in by the 1st of November, 1825.”

Sixty memoirs were the result of this first appeal to the zeal and intelligence of the officers : of these, thirty-five were devoted to the first question ; seven to the second ; and eighteen to sub-

jects selected by the writers themselves, conformably to the latitude permitted by the terms of the third question.

Of the thirty-five memoirs, having for their object to contrive an instrument to verify the correct centering of guns, not one of the instruments proposed, or of the methods of procedure recommended, was deemed by the Committee to satisfy all the required conditions, preserving at the same time sufficient simplicity for use; the premium for this question was consequently not awarded. Three, however, of the memoirs were judged worthy of honourable mention, and of having the names of the writers ascertained; to each of whom, by a subsequent special instruction from M. de Clermont Tonnerre, the present Minister at War, a sum of 500 francs was presented. In the *Mémorial* is inserted a brief analysis and classification of the principle of the methods, by which the several writers proposed to attain their purpose, omitting the technical details. In six of the memoirs, the axis of the exterior surface was taken as the basis of verification; and in twelve others, the axis of the interior surface, or bore, was made the basis: of the latter twelve, seven proposed to effect the verification by an apparatus borne by metallic rods, and five by the employment, as the axis, of a ray of light, which joins to its rectilinear property, the additional advantage of not bending by its own weight, which is the inconvenience to which metallic rods are subject. The three memoirs which are honourably mentioned belong to this class,—one to its first, and two to its second subdivision. The remaining seventeen memoirs, classed together, are distinguished by the common quality, of proposing to effect the verification with the gun placed in a vertical position, so as to avoid the liability to flexure in the apparatus employed.

The committee announce that it has become unnecessary to submit the same question to a second year's consideration, as the model of an instrument has been presented, subsequently to the period named for the reception of the memoirs, approaching in its principle to those contained in the first of the above classes, and which, from the apparent simplicity of its operation, has been judged worthy of being made of full size, so as to be submitted to experiment.

Of the seven memoirs relative to Ricochet fire, one has been judged very superior to the other six, and of sufficient merit to justify the award of the premium of 1500 francs. The author is Lieut.-Colonel Lyautey, second in command at Strasburg, one of the head-quarter stations of the French Artillery; his essay is printed in this number of the *Mémorial*, and is spoken of in terms of high commendation in the report of the Committee on the adjudication of the premiums. We propose, in the sequel, to give a brief analysis of the arrangement and contents of this essay, not less in regard to its intrinsic merit, than in considera-

tion of the interest and distinction which attach to it at this moment in France.

Of the eighteen memoirs devoted to the third question, several were considered as not coming within the proposed limits, as they treated on subjects of the military art having but a very indirect relation to the service of the Artillery. Others presented useful views; but none amongst them possessed sufficient general interest, or was drawn up in a sufficiently able manner, to justify an award.

Thus, then, in the first year, though sixty memoirs have been presented in competition, but one premium has been adjudged; a wise frugality, that, in withholding, raises the value of the distinction sought, and calls forth increased exertion to obtain, what is not too readily given.

We conclude this part of our subject with the questions proposed for the second year. In consequence of their importance, and the extent of research which they will require, the committee have decided on passing over the present year, and have appointed the 1st of September, 1827, as the day on which the memoirs relating to them are to be completed and presented.

" 1. To discuss the respective employment of horse, water, and steam powers in the machines employed in the various departments of the Artillery, viewing their suitability to particular branches, or the influence of circumstances on each in different localities. To indicate in each case the mode of transmitting the force, the most simple, the least liable to derangement, offering the least resistance to be overcome, and presenting the greatest facility of regulation, or of being moderated, at will. To apply the result of this investigation to some one of the several machines employed in the departments of construction; adopting, for that purpose, any of the three powers above mentioned, and examining the several effects of friction, re-action, and, in general, of resistance of every kind, which is a physical consequence of the composition and movement of the machine. To investigate the relation between the force employed and the effect produced, both theoretically and practically, resting the latter on satisfactory experiments. The memoirs to be accompanied by plans of the machine, and by numerical and relative calculations.

" 2. To discuss the circumstances in which the fire of case-shot has a preference over that of round shot or shells; and the distances at which case shot produce the most advantageous effect. To determine the size, the number, and the arrangement of the balls best suited to each calibre and to each kind of ordnance; as also the form and thickness of the bottom. To point out the nature and qualities of the metal most proper for the balls. To show the most advantageous proportion between the weight of the case-shot, and the charge, deduced from experi-

ments already made, or to point out those which it may be desirable to make for that purpose. To compare, in all respects relating to this species of fire, the customs of foreign powers with those of France.

“ 3. To establish, for the cartridges of infantry, the relation that ought to exist between the weight of the ball, the nature and weight of the powder, the windage, the bore and the length of the barrel, and the whole weight of the musket, in order to obtain the most advantageous range, diminishing at the same time, as much as possible, the recoil. To investigate the effect of force in ramming down the charge, on the range and on the recoil. To compare, under all these heads, the arms of foreign infantry with those of the French infantry. To examine the principal experiments hitherto made on the subject of this question, and, if they shall appear insufficient, to submit a scheme of such experiments as shall enable its complete solution. To investigate the advantages on the one side, and the inconveniencies on the other, of having cartridges of two kinds, differing in weight and calibre; one for the infantry, and the other for the cavalry. To compare the arrangements of foreign powers, in this respect, with those of France.”

Previous to a more particular notice of M. Lyautey's essay, it may be proper, in regard to our general readers, to premise a brief explanation of Ricochet fire. It will be easily understood, and readily conceded, that the first practical accomplishment of an Artillerist is, that he should be able to hit his mark. When the mark is in sight, and presuming it to be within the ordinary range of the piece of ordnance that he employs, this is a very simple matter; requiring nothing more than that the gun should be loaded with the usual service charge, that it should be pointed, and that it should be given an elevation proportioned to the distance. The elevation, corresponding to different distances, is, here, the only demand upon the head of the Artillerist: its progression has been determined, by experiments long since made, for all the different kinds of ordnance in usual employ; and as these are not very numerous, and as the progression is in itself simple, all that is necessary in practice is very soon and very easily committed to the memory, and readily recalled at the moment it is wanted. It is far otherwise, however, in respect to simplicity, when the object which is to be fired at is not in view, although its direction may be sufficiently known; when, for example, the view is intercepted by an intervening obstacle, such as the summit of a hill, or a mound of earth, or a parapet artificially raised, protecting from direct fire, and making it necessary that the projectile should not only clear the intervention, but that, at the instant of clearing it, its path should make that particular

angle with the horizon, which may cause it to descend upon the object stationed at a greater or less distance behind its cover. The occasions in which this kind of fire is required, present an almost infinite variety of cases, for which it is impossible to be provided in the memory, and which require to be met by an application, on the spot, of the principles of what is usually called the Theory of Projectiles. We shall endeavour to give a brief explanation of the process by which this application is accomplished.

The only point in which all the cases agree, and it is, apparently, the only generalization of which the subject is capable, is, that the projectile must be in the descending branch of its flight, or trajectory, when it passes the obstacle. Let it be supposed, then, to pass close over, but without touching, the crest of the parapet or other intervention: it will be obvious that the angle with the horizon, at which the remainder of the descent must be pursued, will need to be greater or less, according, first, as the object to be struck is more or less near its protection, and, secondly, as the difference of level between them is greater or less. Further, that as, for the short remainder of the descent after the projectile has passed the intervention, the path will not differ significantly from a tangent to the curve of the trajectory at the instant of passing over,—so, the horizontal distance between the object or point to be struck, and the parapet or other intervention, being taken as the base,—and their difference of level as the perpendicular, if we divide the perpendicular by the base, we obtain the tangent of the angle, which the path of the projectile must make with the horizon when passing over the parapet, in order that it may descend on the object. This angle, technically called the angle of arrival, and which is thus readily deduced from the relative position of the object and its protection, furnishes the means whereby may be respectively computed,—the angle of departure, or that to be given to the projectile at the commencement of its flight,—and the velocity (called the initial velocity) with which the flight must be commenced, in order that the projectile may reach its destination.

Were it not for the resistance of the atmosphere, the path of a projectile would be a parabola; in which case (presuming no considerable difference of level) the angles of departure and of arrival would be identical. The atmosphere, however, by opposing itself to the impulse communicated by the powder with more or less force, according to the velocity of projection, whilst the action of gravity remains uninfluenced, causes the angle of arrival to be always somewhat greater than the angle of departure. Even from this first view may be derived one of those broad practical rules, which, though very far from the precision easily attainable by calculation, is the more esteemed in practice; be-

cause, in spite of the utmost precision, there is, and ever must be, a degree of uncertainty in the flight of projectiles, arising from causes which are not reducible to control. It may be considered, then, as a rule, applicable in all cases where no material difference of level exists, that the angle of departure should always be somewhat less than the required angle of arrival; but not more than a little less; even if the same angle be taken, the error, whatever may be its amount, will be on the safe side; since it must be obvious, that, at any greater angle of descent than the precise one obtained by the tangent, the projectile may still strike the object; but that, at a less angle of descent, it cannot, even by accident, do so: since those projectiles which have not force to clear the parapet, will be interrupted by it in their course; whilst those which clear it, having a more flattened or horizontal descent than will reach the object, must necessarily pass beyond it. Even this rough deduction, however, ceases to apply when the path of the projectile originates at a level differing by more than a few feet from that at which it is to arrive*; and it becomes necessary in such cases, and is safer in all if opportunity permits, to compute the proper angle of departure, as well as the initial velocity, by means of the Formulas of the Theory of Projectiles; having as elements, the angle of arrival, the horizontal distance of the gun from the parapet or other obstacle, and their difference of level. The angle of departure is then given by the inclination of the axis of the piece; and the initial velocity by proportioning the charge according to a ratio dependent on the strength of the powder; which ratio requires to be established by experiment by every State for its own service powder, and which cannot be too carefully done in the leisure and preparation of peace.

We may, perhaps, make ourselves better understood by illustration than by mere description. We will suppose that the face of a bastion is to become the object of attack; and that it is desired to dismount the ordnance stationed on the rampart, in order to remove the impediment which they would otherwise present to the construction of the batteries designed to effect a breach. In the present very improved state of the art of fortification, the guns on the face of a bastion are too well protected by the parapet, and the parapet by the exterior defences, to be much endangered by a direct fire. But by establishing a battery in the prolongation of the line of the face, and so ordering

* The following practical maxim is taken from the notes appended to M. Lyautey's essay. "The charge and elevation being known for any particular distance when the gun and parapet are on the same level, the same charge and elevation may be employed, so long as the difference of level does not exceed one-twentieth of the horizontal distance between them; the elevation being given, in such case, by the tangent scale, instead of by the quadrant, and the gun being laid by it at the parapet, whether above or below its own level."

the flight of the projectiles, that they may pass over the parapet of the face adjoining to the one attacked, the whole length of the rampart on which the guns are placed becomes exposed to the fire, except a very small part immediately behind the parapet passed over, which will still remain inaccessible. We will suppose the length of the face to be 300 feet, and the height of the parapet above the rampart 7 feet; in such case the smallest angle of descent applicable to the occasion, would be that which should cause a projectile passing over the parapet to graze for the first time at the further extremity of the rampart; here the perpendicular is 7 feet, and the base 300; $\frac{7}{300} = .00233$ the tangent of $1^{\circ} 20'$, the required angle of descent. This is the minimum limit, and presumes that the projectile has fulfilled the first condition, of passing close over the crest of the parapet. The maximum limit must be governed by the greatest inclination that the carriage will permit to be given to the axis of the piece; since every angle of descent greater than $1^{\circ} 20'$ will strike some part or other of the rampart, and the greater the angle, the nearer of course to the parapet. About 16° may be considered the greatest elevation which the usual carriages of ordnance will admit, and which will give a somewhat greater amount in the angle of arrival. In order to estimate what space of the rampart, situated immediately behind the parapet, will remain protected even at the greatest elevation, we will take the angle of arrival at 18° ; the height of the parapet 7 feet, divided by .0325 the tangent of 18° , gives 21 feet as the distance from the parapet, at which a projectile, having an angle of descent of 18° , may strike the ground supposed to be 7 feet below its crest or summit. Within these limits every elevation, having its corresponding charge, is applicable to strike some part or other of the rampart; the projectiles which passing close over the parapet graze towards the farther extremity of the rampart, will prove the most destructive, not only because their flight ranges along the whole length of the rampart, but because the greater initial velocities, and, therefore, the greater terminal velocities, correspond to the lesser angles of elevation, and give a greater force at the period of arrival. Those are not less efficacious, which, by a proper calculation of the angle of descent, are made to graze at a less distance from the parapet, and to make one or more subsequent grazes before they reach the farther extremity of the rampart; the angle being so ordered, that in the space between the grazes, the projectile may not rise higher than the line of its destructive agency. (It was these successive bounds, called *ricochets* in French, which caused the name of the *tir à ricochet* to be applied, at the period of its first invention, to this mode of silencing the fire of a work. At that period, the absence generally of *traverses*, of which we shall presently speak, occasioned works to be more liable to the action

of *ricochets* than they have since been rendered.) Pursuing, however, the particular case of attack that we have supposed, it remains only to remark, that, by a judicious employment and combination of all the means, *i. e.* of all the angles, within the limit of applicability, every part of the rampart, from about twenty feet from the parapet to the extremity of the face, may be visited by its most appropriate fire, and may be thereby rendered uninhabitable, and its guns dismounted.

We have hitherto proceeded on the supposition, that the work which is attacked presents no obstruction to the range of projectiles from its one extremity to the other—a case of rare occurrence in modern works, in which it is usual to find the rampart broken into spaces, separated from each other by traverses, or mounds of earth of seven or eight feet high, thrown up across it; each space being generally of sufficient magnitude for the station and exercise of two pieces of ordnance. Here the fire available to the purposes of the assailant is reduced within much narrower limits; having to search, as it were, the interior of these spaces, under the double protection of parapet and traverse; reducing very low indeed the chance of accidental success, where the principles of the art have not been specially consulted, or where, indeed, they have not been followed out with an approach, at least, to precision. In such cases, and having in view the effectual silencing of the fire by the destruction of the carriages and platforms of the ordnance, all the lower angles are of course excluded; shells passing over the parapet at small angles of descent may do more or less injury to the traverses, by lodging in them, and producing by explosion the effect of mines: they may also disturb the men employed in serving the ordnance; but the least angle of arrival, by which the ordnance themselves can be reached, is when the projectile, passing over the one traverse, descends on the more distant carriage of the two situated behind it. Now, supposing the space between the traverses 36 feet, their height 7 feet, and that 2 feet above the ground be allowed, as being about the middle of the profile of a carriage, we have the perpendicular 5 feet, and the base 27 feet, whence the tangent of the angle of arrival = .0185, and the angle itself $10^{\circ} 30''$, as the least angle that it would be safe to employ. The carriage nearest to the traverse will be found by a similar process, to be only attainable by the angles corresponding to the greatest elevation that can be given to the ordnance. It will be remembered, that every different elevation requires its corresponding initial velocity, varying with every alteration in the distance from the work attacked, and with every difference of level between the pieces of the assailants and the summit of the parapet; and that it is, moreover, different for every different kind of ordnance.

We think that we have said enough to show that the kind of fire, of which we have endeavoured to convey a brief, and therefore we fear but a very imperfect notion, embraces too great a variety of cases, and involves details too numerous to be supplied by memory on the spot, whether from the impressions of previous study, or from the recollections of former practice. Varied as are the occasions which the skill of the Artillerist is called upon to meet, the means within his command are scarcely less varied or extensive. By duly proportioning the charge of powder,—from the smallest quantity that, when fired, will produce certain discharge and give a sufficient force against men and carriages, (being two and three ounces for the ordnance that are most generally employed,)—to as many pounds as are equivalent to a third of the weight of the projectile,—he can regulate the initial velocity at his pleasure, from a little more than two hundred feet in a second, to upwards of fifteen hundred feet. By giving a suitable inclination to the axis of the piece, chosen between 16° of elevation and its point-blank or horizontal direction, or if need be so, by a depression of several degrees, (applicable in cases where the mark is at a lower level than the gun,)—and by accompanying the inclination by a proportioned charge,—he is enabled to clear any ordinary intervening obstacle whatsoever, at such an angle of descent in the flight of the projectile, as may cause it to strike an object protected from direct fire and from view, whether it be situated near or far from its protection, and whatever may be their difference of level; as well as to reach his mark, either at the first graze, or after any number of successive grazes, the path and velocity of which are in great measure subject to his regulation.

In what has been said we would not be thought to have overlooked the irregularity in the flight of projectiles, arising from occasional variation in the strength of the powder, or in other similar and accidental causes, which must at all times render the effect which we would wish to produce more or less uncertain, notwithstanding the utmost precautionary pains. But this affords no valid argument for neglecting to take every pains in our power that may contribute to success. He who is at the trouble to calculate the precise angle and charge which ought to produce the effect he wishes, will commence his fire at the point, where another, who begins at random, and depends on observation of his own failures for successive approximation, does not arrive perhaps until hours of valuable time, and means yet more valuable, have been wasted; or at which, perhaps, he may not arrive at all, for his observation may only tell him that the projectile passes over the parapet and enters the work; but whether at the angle which will accomplish the purpose he has in view within the work, may not be in the power of observation to

determine. Here, as in most cases, theory and practice have their distinct and appropriate offices. It is the province of theory to determine the limits of fire applicable to an occasion; it is the province of experience to select from between those limits the fire that may be most effective.

The invention of this method of silencing a work attaches to a no less eminent personage than Marshal Vauban: and it is not a little remarkable, that the same individual to whom the art of defence is so much indebted,—who by reducing the height of parapets and covering them with exterior works, rendered them almost inaccessible to direct fire, and consequently almost perfect against the then practised means of attack,—should also have conceived and introduced an improvement in offensive warfare, restoring its accustomed superiority over the resources of defence, which the inventions of his own genius had otherwise endangered. And it is scarcely less remarkable, that two of the principal characteristics of the system of defence which he originated, viz., the reduction of the Relief, and the extension in the length of the Faces, are precisely those qualities, which render works constructed according to his system peculiarly liable to the successful operation of Ricochet fire.

We have applied the same extended signification to the *tir à Ricochet* as that in which it is understood, and is discussed in the Essay of M. Lyautey; although he is not without occasional checks, from a precision, which would limit the more strict application of the term to the particular case in which the effect is sought by successive grazes. To make good ricochets, the angle of descent ought not to exceed four or five degrees; and at angles of double that amount, or a little more, they either cease altogether * or become of no practical avail. Every increase of elevation beyond that amount, is an advance towards a vertical fire, but has no precise term of designation. The French have recently introduced the terms of *Ricochet mou* and *Ricochet tendu*, the first applicable to the trajectories of greater curvature, and the second to the more flattened curves. The *Ricochet mou* commences at the greatest elevations which the carriages will admit, declines progressively as the path becomes more and more flattened, until it passes into the *Ricochet tendu*, but without any definite line of demarcation being established between them. The *Ricochet tendu* in like manner passes through the point-blank and the first angles of depression, until it again passes into the *Ricochet mou*, but with a line of separation as little defined as before; the *Ricochet mou* then continues, until its ulterior limit is found in the

* The limit of Ricochet is theoretically governed by the diameter of the projectile, the angle of departure, and the initial velocity; but in practice it is greatly influenced and determined by the nature of the soil.

inability to depress further. It would not appear that much is gained by the employment of these terms, either in correctness of expression, or in precise definition; we believe that they have not been adopted, as yet at least, by any other nation than the French.

We may now proceed to the consideration of M. Lyautey's Essay, which is divided into three sections, and is accompanied by an extensive series of tables. The first section discusses the respective offices of Direct and Ricochet fire in sieges and in field-service; the second, the nature and design of the tables; and the third, the state of instruction in France relatively to Ricochet fire, concluding by proposing a scheme for its amelioration.

In the attack and defence of regular fortifications, the proceedings on both sides, and the most ordinary and probable situations in which either party may be placed, can be sufficiently anticipated, to enable the establishment of precepts of general application, subject to some slight modifications which circumstances may render necessary. Accordingly M. Lyautey has occupied the greater part of the first section in tracing the usual operations of a siege, both in attack and defence, and in examining the opportunities which they afford for the most advantageous employment of Direct and Ricochet fire, whether Ricochet, properly so called, or at the higher angles when the first descent is the sole consideration. This part of the subject has been so much practically studied, as to leave little opportunity of saying anything very new or very striking; but those who may desire such instruction, will find in the Essay a clear and able exposition of the services which both kinds of fire are respectively called upon to perform on such occasions.

The attack and defence of field-works admit also of general precepts, but necessarily of more uncertain application, and subject to more extensive modification. The purposes, which field-works are designed to serve, frequently require their construction at greater heights above the surrounding country than form the site of regular fortifications, and we have seen that differences of level are peculiarly influential on Ricochet fire. In investigating the effect of different heights in modifying its application, M. Lyautey is led to the following practical conclusions: that if the height of the work exceeds fifty metres (a little more than fifty yards) above the level of the attacking guns, it is useless, with field-pieces fired with the service-charge, to endeavour to produce good ricochets: and that to fulfil that condition, the pieces ought not to be at a less distance from the work than the French point-blank range, (i. e. the English line of metal) augmented by ten times the amount of the difference of level between the guns and the work. For the fire of the work itself, he concludes, that taking 6° of depres

sion as the maximum which the present construction of the carriages will admit, and an angle of descent of 10° as the limit of Ricochet, if the troops are nearer the work than ten times the difference of the level of the ground on which they stand, and that of the guns of the work, they are not subject to Ricochet; and that the farther they retire from this distance, the more destructive to them that fire will become, until they are altogether out of the range of the ordnance.

The employment of Ricochet fire against troops and artillery in the field is still less liable to have its occasions anticipated; M. Lyautey confines himself, accordingly, to general instructions on the choice of stations for artillery in the field, so that it may not lose by its position any of the properties of its different modes of fire; noticing particularly the common error of placing guns in too great command over the surrounding country, whereby their fire is rendered too plunging for its most advantageous effect. He concurs with those who, instead of the ordinary method of reaching troops at the first graze, prefer, wherever the nature of the intermediate ground will admit, to fire at low angles, little removed from the point-blank, so as to graze at some distance short of the enemy, and to reach him in Ricochets of very flattened curves; considering not only the greater moral effect which it is calculated to produce, but also that it avoids the occasional failure, caused by the uncertain range of projectiles in successive rounds with the same service-charge; particularly with light howitzers, which he justly characterises as very uncertain indeed in that respect.

M. Lyautey has not noticed the consideration of the properties of Ricochet fire, in assigning the most advantageous height above the sea for batteries for the defence of coasts; we were struck by the omission, because this consideration does exercise a very important influence in determining the position of such batteries, and the general precepts which it enjoins are but little liable to modification in different localities.

The object of the extensive series of tables which M. Lyautey has appended to his Essay, is to supply, by inspection on the spot, all that calculation has heretofore been required to furnish: so that knowing the relative positions of the guns to be fired, the mark to be struck, and the intervening obstacle, it shall only be necessary to open the tables, to obtain at sight all the particulars required to regulate the fire.

The tables are computed for seven distances, viz., for 300 toises, 280, 250, 200, 170, 150, and 130 toises, which are considered to comprise the space within which ricochet fire, understood in its extended sense, is most usually required; the two first distances are also specially selected as applicable in the first parallel, and the three last in the second parallel in sieges. For each of these distan-

ces, and for every kind of ordnance suited to the employ, the angles of elevation are inserted, corresponding to every 25 feet of increase in the initial velocity, commencing at 225 feet in a second, and continued until the elevation has diminished to the French point-blank; from thence the angles are only given corresponding to every 50 feet increase to a further limit, determined either by the charge to produce it amounting to one-third the weight of the shot*, or by the condition of reaching at the first graze an object two and a half feet in height, (for example, a gun-carriage,) situated at the extremity of a face of 50 toises in length; being the most flattened curve that is likely to be required.

All these particulars are separately given; first, when the gun, and the mark it is pointed at, (the crest or summit of the intervening obstacle,) are on the same level; next, when a difference of 12 feet in the level exists between them; and subsequently, for differences increasing, by 6 feet at a time, to an ultimate difference of 42 feet.

For all the cases that have been thus severally enumerated, the tables contain the mutual relation between the following quantities, viz., the initial velocity; the angle of departure; the height in inches and lines to which the tangent scale must be raised if the elevation be given by it; the velocity of arrival; the angle of arrival; and the tangent of the angle of arrival. These quantities have been computed agreeably to the Formulas contained in the *Théorie de Balistique* of M. d'Obenheim, the treatise of the kind most esteemed in France; and by the aid of an instrument invented by M. d'Obenheim, and known by the name of the "*Planchette du Canonier*," which solves mechanically the various problems relating to the flight of projectiles in all cases of firing upon horizontal planes, and greatly facilitates the calculation when the extremities of the trajectory are not on the same level. The inventor considers that, after a little practice with this instrument, the error of its solutions need not exceed three inches in the initial velocities, and nine seconds in the angles; and M. Lyautey is of opinion that his tables do not include errors from that source greater than double, or at furthest than four times, the amount estimated by the inventor. The tables are now in course of very careful verification, under the direction of the Central Committee, for which their printing has been delayed; it is understood, however, that the impression will be complete, and the tables ready for distribution about the close of the present year.

The ratio of the charge of powder to the initial velocity is not hitherto included in M. Lyautey's tables. He does not consider that the ratio has been yet determined by sufficiently careful or

* For howitzers, the charge that fills the chamber.

extensive experiment for a matter of so much importance ; and he views it also as a desideratum which is on the eve of being supplied under the direction of the Government. This ratio may be very easily appended subsequently, for the different kinds of ordnance ; when the tables will be in all respects complete.

M. Lyautey esteems the state of instruction in France relative to Ricochet fire as defective, inasmuch as the knowledge it is calculated to produce is limited to the impression which the particular case or cases of practice, passing under the notice of individuals, may make on their minds ; when, by a more suitable arrangement, it might lead to more extended consideration, and develope, as it were, to general apprehension, the principles on which the practice will require to be modified, so as to meet the various exigencies occurring in war. He contemplates the service his own tables appear well calculated to render, in furnishing the means of tracing the connexion between different cases of practice, which would otherwise remain as insulated facts in the memory. He suggests, that the instruction of the polygons should be particularly directed in each year to the illustration of some part or parts of the tables ; by which he anticipates also the incidental advantage of having them submitted to a practical verification. And for the better illustration of the effect of differences of height, he proposes the construction of two polygons, the parapet of one being on the same level with the guns, and of the other eighteen feet above them ; when, by varying the length of the faces in different years, and by stationing carriages in different parts of the work as objects of the fire, and by variously protecting them by traverses, he considers that a sufficient exemplification would be afforded of the cases occurring in sieges. For practice at the greater differences of level required in the attack and defence of field-works, he proposes the construction of a parapet on the top of the butt, with sufficient space for two carriages to be stationed under its cover, at about seven feet below its crest ; and that this work should serve not only as a mark for fire, but that it should become, in its turn, the station of guns, to practice Ricochet at angles of depression.

Not the least interesting or important part of the *Mémorial* is the portion allotted to a description of the changes which from time to time shall be introduced in the system of the *materiel*, or in other branches of the service of the Artillery. The present Number contains a succinct account, arranged under different heads, of the principal objects of improvement which have occupied the attention, in the last year, of the Central Committee, to whose judgment all suggestions are referred, and under whose superintendence those which are approved are carried into effect. A new system of Pontoon exercise and manœuvres, rendered ne-

cessary by a recent reform of the establishment and an alteration in the equipment of Pontoons, has been submitted by a board of officers of the corps specially appointed for the purpose, and has been approved. Several minor improvements relative to small arms are detailed, principally in regard to the system of their repair and issue. An alteration in the construction of the carriages and waggons of the field-artillery had been for some time in contemplation in the French service; and carriages on a new model were submitted to trial in the years 1824 and 1825, at the head-quarters of five of the regiments of Artillery; the essential differences from the old construction, (that of Gribeauval,) consisted in the mode of attaching the limber to the gun-carriage; in making all the wheels of the same height; and in the limber of the gun and waggon being alike, and of course interchangeable where occasion may require. All these improvements are adopted, with modifications, from the British service; and we remark with pleasure the readiness with which the superiority that the British carriage-department had attained is admitted. The reports from the five head-quarters were unanimous in approval of the new construction, except in regard to the mode of draft, it having been proposed to substitute the English custom of shafts for that of the pole, used previously in France. On the expediency of this change a difference of opinion is stated still to exist; and in consequence nine brigades, consisting of different field-ordnance in pairs, one of each being fitted with a pole and the other with shafts, have been sent to the nine regiments of Artillery, including that of the guard, for another year's trial; and various improvements in the harness have been submitted at the same time. A new carriage for the Artillery employed in sieges, which has been some time on trial, is finally approved, and its adoption ordered. The model of a carriage for heavy ordnance to replace the garrison carriage of Gribeauval, and to be at the same time applicable to batteries on the coast, is described, and is put in course of trial. But the most important service, in their own view, on which the Committee has been employed in the past year, has been a thorough remodelling of the courses of practical instruction, which the officers of the Artillery are required to pass through, after they have completed their education in the elementary schools, and have joined the head-quarters of their respective regiments. Two-thirds of the officers entering the French service receive a professional education in the schools provided by the government. Distinction, as children, in the provincial schools, (which are annually visited by a committee of examination for that purpose,) recommends to an appointment to the Ecole Polytechnique, where, with the exception of the principles of civil architecture, we believe the instruction is exclusively confined to the mathematical and physical

sciences in the abstract; in these a considerable advance is required, as two years is accounted the shortest period in which it can be made by the ablest and most diligent. On quitting the Ecole Polytechnique, each individual, according to the profession, or branch of a profession, for which he is destined, joins a particular school of *application*, as it is termed; i. e. where the general principles of the sciences that he has acquired are taught in their specific application to the profession to which he is to belong. This school for the officers of artillery is at Metz, where they remain two more years, during which time they have the rank of sous-lieutenant. They are then appointed to one or other of the regiments of Artillery, and join its head-quarters. There are eight regiments having their head-quarters respectively at Strasburg, Metz, Douai, Toulouse, Rennes, Auxonne, La Fère, and Valence, exclusively of the Artillery of the guard at Vincennes. Joining with the rank of sous-lieutenant, they receive the commission of lieutenant at the expiration of the first year, but remain under instruction for the first three years, unless their services are called for intermediately, to accompany the part of the regiment to which they belong in campaign; in which case, however, they are obliged to complete the period of three years on their return to head-quarters; nor can this condition be dispensed with, except when a lieutenant gains, during his absence, the rank of captain, either by distinction or by succession*. It is for this period of three years, that the general instruction, at what are termed the *regimental* schools, has been revised by the Central Committee, and a scheme, embracing and arranging the whole, has been submitted to the government, approved, and is ordered to be adopted at each of the regimental head-quarters. This scheme, arranged in synoptic tables, is printed in the *Mémorial*, and particularizes the different subjects on which the officers, non-commissioned officers, and soldiers are to be respectively instructed. A distinction is made between the two-thirds of the officers who join from Metz having passed through the elementary schools, and the other third, whose previous education is presumed to have been less carefully attended to, and who have, therefore, occasion to learn much with which the others are already acquainted; accordingly, although a great portion of the instruction is common to the officers generally, certain courses are allotted to each of the above classes, suited to their respective qualifications. Such subjects as will admit of practical illustration, such, for example, as the duties of Artillery in sieges, are studied at the Polygons, which are works constructed of the proper form and dimensions, in which the proceedings in attack and defence are practically gone through; but of necessity the greater

* Promotion in the French Artillery is given, two-thirds by selection, and one-third by seniority.

part of the subjects require to be treated didactically, for which purpose the form of lectures is preferred. Under the advice of the Committee, thirteen distinct courses, all on subjects of primary importance, are in preparation by individuals selected from amongst the officers and from the professors attached to the schools, and are expected to be completed in the present year. Not content with the bare indication of the subjects, the committee have themselves pursued the consideration of each, so far as to specify the number of lectures that it shall occupy, and to prescribe the outline of the contents of each particular lecture; all which detail is printed in the *Mémorial*; and by a special direction of the Minister-of-war, the officers of the Artillery generally are invited to compete in filling up the courses with the individuals to whom they have been severally intrusted. The outline of the thirteen courses occupies sixty-eight pages, and we regret that our limits oblige us to confine ourselves to the specification of the several subjects, and to the outline of one of the courses; which will, however, serve to shew the comprehensiveness, as well as the detail in which each subject has been viewed by the Committee. We shall select the course on the manufacture of gunpowder, as being, perhaps, the least technical, and therefore the best suited to be submitted to general judgment.

Subjects of the Courses.

1. On the Theory of Projectiles. Eight lectures.
2. On the Construction of Ordnance. Eight lectures.
3. On the Construction of Gun-Carriages, and of other Carriages appertaining to the Ordnance. Seven lectures.
4. On the Construction and Employment of the various Machines required in the Arsenal, Laboratories, and Manufactories belonging to the Ordnance. Ten lectures.
5. On the Processes adopted in smelting, working, and forging Iron, for its various application in the Service of the Ordnance. Eight lectures.
6. On the Founding of Cannon. Ten lectures.
7. On the Manufacture of small Arms. Twelve lectures.
8. On the Manufacture of Gunpowder. Eleven lectures.
9. On the Service of Artillery in the Field. Eight lectures.
10. On the Service of Artillery in Sieges. Eight lectures.
11. On the Service of Artillery in the Défence of Fortified Towns. Nine lectures.
12. On the Service of Artillery in the Defence of Coasts. Three lectures.
13. On the Construction of Batteries. Six lectures.

OUTLINE OF THE COURSE OF INSTRUCTION ON THE MANUFACTURE OF POWDER.

1st Lecture. Organization of the Department.

Board of Directors. Committee of reference. Enumeration and de-

scription of the powder-mills, and refineries of saltpetre in France. Their interior organization and administration. Duties of a "Commissaire de Poudres." Duties of the officer of Artillery, Inspector of the establishment.

2d Lecture. Preparation of Saltpetre.

Method of collection in France by the government collectors. Public laws on the subject. Method of its collection in different countries of Europe stated and discussed. Materials containing Saltpetre. Means of ascertaining them. Washing of the materials. Salts contained in the water after the washing. Their properties and proportion to the materials. Modes of saturating the water employed in the washing. Its evaporation. Furnaces and boilers used in the process. Crystallization of crude saltpetre. Mode of treating the mother water. Of the froth and scum. Extraction of sea and other crystallizable salts.

3d Lecture. Refining of Saltpetre.

Reception of crude Saltpetre into the refineries. Process of refining. Preliminary washing in water previously saturated. Refining, properly so called. Crystallization. Washing of crystallized Saltpetre in troughs or tubs. Desiccation. Test of pure Saltpetre by nitrate of silver.

4th Lecture. Sulphur and Charcoal.

Sulphur. Its origin and extraction in a crude state. Volatilization. Process followed in the Refinery at Marseilles. Theory of the process. Charcoal. Cultivation of the black Alder (*Frangula*, Lin.) Public laws relating to it. Woods which may be substituted in the charcoal required in the manufacture of powder. Choice according to age and size. Process of making charcoal. In open ditches. In iron or copper kettles. In ovens. By distillation in close vessels. Comparison of the product and cost of each process. Properties of the charcoal respectively obtained. Preservation of charcoal.

5th and 6th Lectures. Manufacture of Powder.

Different kinds of powder used in France. Proportions in which its component parts are employed. Proportions employed in other countries. Trituration of the material mixed, or unmixed. By pestles and mortars. By mill-stones. By other mechanical means, (*Par les tonnes et gobilles.*) Mixture of the materials when they have been triturated separately. Caking. Pressure to produce it. By pestles. By screw, or hydraulic presses. By vertical mill-stones. By the *laminoir*. Granulation. Sieves of parchment or metal. Grooved cylinders. Mode of rendering the grains spherical. Separation from the dust. Equalization of the grains. By sieves. By the bolter. Glazing. Process of drying. Naturally, in the open air, or covered. Artificially, by Champy's ventilator. By the apparatus used at Essonne. By steam. By stoves. Dusting, and ultimate equalization of the grains. Detailed description of the machines used in all the above processes. Calculation of what each will perform in a day's work of ten hours.

7th and 8th Lectures. Examination and Proof of Powder.

External appearance. Form and size of the grains. Round or regular. Glazed or unglazed. Hardness and density. Mode of determining the density. Description and use of the gravimeter. Discussion of the preceding particulars, in relation to the service for which the powder is required. *Eprouvettes*. Description of those used in other nations, as well

as in France. Division into two classes. Proof by volume. Proof by weight. Reasons why the proof by weight is considered superior. Of the mortar éprouvette, and of other instruments used in the verification of powder. Rules under which new powder, and that which has passed through the mills a second time, are received in the service. General discussion on the modes of proof.

9th Lecture. Preservation of Powder.

Causes of its deterioration, and of the methods of counteracting them. On the form and dimensions of powder-magazines. Modes of stowage. Of barrelling. Dimensions of barrels used in France. On the mode of packing the "poudre de chasse," and the "poudre de commerce." Precautions against damp in magazines. Against lightning. Theory and description of conductors. Regulations relative to the transport of powder.

10th and 11th Lectures. Theory.

Chemical examination of saltpetre. Composition. Properties. Formation. Artificial saltpetre beds. Discussion of their advantages and products. Chemical history of sulphur. Its action in powder. Physical and chemical properties of charcoal. Indication of the properties most essential in the charcoal to be used in the manufacture of powder. Analysis of gunpowder. Theory of its combustion. Nature and quantity of the gases produced. Inquiries that remain to be made as to the absolute force of powder, and to the quantity of heat developed in its combustion.

The courses, when completed according to the outline prescribed by the Committee, and in the manner which we have described, will form the basis of the instruction to be communicated on each particular subject; and by the mode adopted for its communication, the means are provided for its being kept at all times to the level of such improvements and alterations as may successively be adopted. The French Artillery has two distinct ranks of captain; the junior of which, during peace, and with a few exceptions, do not remain with their Régiments, but are distributed in the arsenals, foundries, manufactories of small arms, &c., in the duties of which they assist for the time, and are required to furnish occasional reports, affording evidence of their attention and proficiency. On promotion to the rank of first captain, and on rejoining the head-quarters, those officers are selected whose reports have been most approved, to read particular courses, accompanied by such additions, (previously submitted to the proper authorities,) as the subject may appear to themselves to require. The selection for the performance of this duty is itself considered as a distinction.

The part of the *Mémorial* allotted to inventions preserved in the Repositories, is occupied in the present number with a description of fourteen different methods of loading small arms at the breech, accompanied by plates, in which each method is represented. We have already trespassed so much on our limits,

that we must confine ourselves to this general notice respecting them.

The attention of the officers of Artillery being called to the subject of case-shot, by its forming one of the prize questions for 1827, the Committee have caused to be translated, and have inserted in the *Mémorial*, extracts from the two most able foreign publications on the subject. Two of the extracts, respectively of twenty-five pages and of forty-six pages, are from the well-known work of M. de Scharnhorst, a Prussian officer; and a third, of fifteen pages, is from a more recent publication by M. de Borkenstein, a Swede. Both the original treatises are in the German language. With these extracts the first number of the *Mémorial* concludes.

E. S.

III. *A Key to the Knowledge of Nature; or, an Exposition of the Mechanical, Chemical, and Physical Laws imposed on Matter by the Wisdom of the Almighty, comprising a brief System of Natural Philosophy and Chemistry; also a New System of Physiology, wherein all the Phenomena of Nature are Explained on Electro-Chemical Principles.* By the Rev. R. TAYLOR. "And God said, let there be light, and there was light." London, Baldwin, Cradock, and Joy. 8vo. 666 pages.

SUCH is the title-page of a work, which our readers will, doubtless, thank us for introducing to their notice. The critic is often called upon to confer praise, and often to dispense censure, but his task is never so agreeable as when it consists in bringing hidden merit to light, and in rescuing the efforts of an original and inventive genius from oblivion.

The author tells us that, having minutely studied Sir H. Davy's masterly researches in electro-chemistry, he was struck with the obvious and easy solution which the doctrine of electro-chemical principles offers to all the difficulties hitherto attending an explanation of the grand operations continually going on in the material universe. "As he traced the various branches of his system," he says, "the elucidation of each particular part threw a light over the whole, so that where conjecture was at first the utmost limit at which he could arrive, he afterwards attained to demonstrative inductions, and has, consequently, had some sections to re-write many times." With respect to the first eight chapters, he says, "he has nothing particular to remark." For his theory of chemistry, "the author is indebted to the splendid discoveries of Sir H. Davy," but with respect to his theory of gravitation and planetary motion, "he is not aware that he has met with

any direct hint in any author." His theory of geology, he tells us, "is founded on Sir W. Herschel's observations on *nebulae*;" and, with respect to "the theories of vegetation and animalization, the facts and experiments," he says, "must bear them out."

We shall follow our author's example in regard to the first eight chapters, and pass them by in silence: the fact is, the less that is said about them the better: and proceed at once to the ninth chapter, which treats of chemistry, a science which he thus introduces to our notice:—

"Chemistry is the science which teaches the investigation of matter, and describes its physical properties. It may be termed the key which unlocks the secrets of nature, enabling us to make her treasures all our own. It is, therefore, both on account of the wonders it displays, and its general utility in promoting all the conveniences and refinements of life, the most fascinating and interesting of all the sciences. In short, the universe may be considered as one vast alembic, wherein the whole mass of matter is elaborated by the two electricities; which, having in obedience to the Almighty *FIAT*, already moulded part of it into spheres, are still under the guidance of Infinite Wisdom, poising and impelling these spheres in their orbits, and exerting a peculiar energy on all their component ingredients.

"Mankind having, by long observation and experience, detected this energy, which they have named Chemical Affinity, can now dissolve, by bringing the two electricities together, the most dense and tenacious substances in nature. Hence they have been enabled to discover, that there are but a few distinct ingredients of which this world, or at least the tangible part of it, is composed."—P. 203.

Our readers will now perceive that the Rev. R. Taylor has mounted an electrical hobby-horse. We shall briefly trace his journey—dwelling chiefly upon the novelties which he encounters in his way. Among the most eminent British chemists he enumerates *Sir Robert Boyle*? Priestley, Black, Bishop Watson! and Lord George Cavendish!! "But no one," he adds, "has made so many, or such grand discoveries in the science, as Sir H. Davy."

"To Sir H. Davy we are indebted for the discovery of the real nature of electricity; and such a discovery it is, that, when the prejudices of the present age shall have passed away, the name of Davy will be as renowned in chemistry as that of Newton is in astronomy. Previously to his decomposition of the alkalies, potass and soda, with the large galvanic apparatus of the SURREY INSTITUTION, this science was but a mere register of some of the habitudes of the electric fluids, and was pursued without any theory or knowledge of their real nature. It was then (in England) generally supposed that there is but one electric fluid, which is

that now called the positive ; and that all bodies contained a certain portion of it, which was considered as their natural share. But when a body was charged with more than this specific quantity, it was then said to be positively electrified ; and when it contained less, it was then considered as negatively affected :—hence the terms *positive* and *negative* electricity. The science therefore treated of only what we shall for the sake of distinction call *uncombined* electricity.

“ But the discoveries of this illustrious philosopher prove, that there are two electric fluids ; so that when a body was in such a state that they pronounced it negatively electrified, or as possessing less than its natural quantity, it was in reality charged with the fluid called the negative. But this is the least part of the discovery. He has established the fact that the electric fluids are identical with the cause of chemical affinity. That one of them is inherent in the one class of the elements ; and the other in the other class ; and that whenever these elements, charged with the opposite fluids, are, under certain circumstances brought together, chemical combination takes place ; the original particles of the one uniting with those of the other ; particle to particle ; forming a new substance, with all its demonstrable properties distinct from either of the two. But this union is only effected by each parting with a certain definite proportion of the electric fluid ; so that it is the violent tendency which these two fluids have to unite, that is the cause of attraction. A portion of the electric principle always remains with the matter, (or it would perhaps be as proper to say, that matter and the electric fluids are of indivisible essence,) which retaining its attraction for the other fluid, constitute the cause of the original particles combining so very closely. The definite proportions of each fluid, thus given out, by combining, form caloric. Should the new substance have a great capacity for caloric, it becomes latent ; if not, it escapes as heat. So that fire, or combustion, is neither more nor less than an example of chemical combination. It is an established law, that when two substances combine they condense ; often passing from a gaseous to a liquid state, or from a liquid to a solid. Heat is likewise invariably produced ; though sometimes solids combine and form liquids, when cold is the result :—the liquid requiring more caloric for its fluidity than the combination generates.

“ Electricity, then, existing as the cause of chemical affinity, is inherent in all substances ; and is as much an essential ingredient as any of the ponderable materials :—for this reason we shall call it combined electricity. Hence, we have two kinds of electricity ; the *positive* and the *negative* ; and they exist in two states, *combined* and *uncombined*, and perform very different offices in the economy of nature. Combined electricity is the cause of chemi-

cal affinity, as it relates to the use that man makes of it in common culinary purposes and chemical operations. It is, besides, the cause of all the natural changes in the mineral, vegetable, and animal kingdoms; and is also what gives motion to the heavenly bodies. Uncombined electricity is the cause of thunder and lightning, earthquakes, hurricanes, many of the winds, with a variety of minor phenomena, all of which will be explained in the following pages."—pp. 208, 209.

We must apologize for this long quotation, but we were in justice bound to give it entire, as setting our author's accuracy as an historian, and his penetration as a philosopher, above all competition. His views of the atomic theory, and of chemical combination, are set forth with corresponding originality and ingenuity; and at page 225, we have a detailed account of the "most famous galvanic apparatus of the SURREY INSTITUTION;" which is taken *verbatim* from Sir Humphrey's description of that constructed many years ago at the Royal Institution, which our Reverend author has somehow or other overlooked.

In Chapter IX. the supporters of combustion are treated of, and some *new* views are developed concerning the cause of heat.

"As caloric, so far as is known, is composed of unvarying proportions of the two electric fluids; and as the integrant matter of our planet, so far as we can ascertain, is composed of nearly equal proportions of negative and positive elements;—we may reasonably conclude, that there is an equal quantity of each kind of elements; or, what expresses our meaning more correctly, that there is the same exact proportion of the electric fluids: so that, was the Almighty FIAT given, the whole of our planet might vanish in a flash of fire and a small globe of crystalline matter, or utter annihilation:—which, if viewed from a distant part of the universe, would only resemble one of those small meteors, that, in a clear autumnal evening, play about in the aerial regions, surprise us with their brilliancy, and then, perhaps, disappear for ever."—pp. 227, 228.

Having given a sample of Mr. Taylor's theoretical accuracy, we now select the following as a proof of his practical accuracy.

"Chlorine combines with oxygen in two proportions, yet not by simple mixture. But when hyperoxymuriate of potash is distilled, a gas is liberated compounded of oxygen and chlorine, which Sir H. Davy called Euchlorine. It is of a higher yellow than chlorine, has the smell of burnt sugar, and is very explosive. The second combination contains more oxygen, and is still of a higher colour. It also combines with nitrogen in the proportion of 91.2 chl. to 8.8 nitrogen:—forming the most dense of all fluids, with a specific gravity of 16.53; nor can it be solidified. It likewise unites with all the simple substances except carbon, which requires the intervention of a third, as hydrogen, to make

them combine; under no circumstance will it combine with charcoal."—p. 233.

In the preceding paragraph, the conversion of oxygen into euchlorine, and the ponderosity attributed to chloride of nitrogen, are not less remarkable than the author's intimate acquaintance with Mr. Faraday's excellent experiments upon the chlorides of carbon. We believe, by the way, that the name of that indefatigable chemist is not once mentioned in the book before us: another proof this of its claims to originality. One or two more practical quotations and we have done. At page 234, we are recommended to *stuff ships with salt to prevent dry rot*; and are told, "that the muriates are the most volatile of all the salts, and yet the least affected by excessive heat, and that with a larger dose of oxygen they form hyperoxymuriates." We fear that Sir H. Davy's admirable researches respecting the muriates have been very hastily perused by this learned commentator.

We have thus selected two or three specimens of Mr. Taylor's theoretical reasoning and practical details; it remains for us to produce one or two of his "embellishments," and a little of his eloquence. After speaking of our method of warming our houses, and contrasting them in detail with those used abroad, he sums up the advantages of our open grates in the following pathetic strain:—

"To our method is perhaps owing, in a great measure, the rosy complexion almost peculiar to the English nation. And the last reason for our continuation of it, and certainly the most endearing to an Englishman, is the effect of custom. He finds in his fire a cheering companion, capable of dissipating all his sorrows, or at least of alleviating his care and anxiety. When the storm rattles without, he has a continual source of pleasure within, from the stirring of his fire. At the blazing hearth he reaps, with redoubled zest, the full harvest of domestic joys;—here, all his affections expand to the full tide of benevolence; here, his patriotism is enkindled, and his love of mankind fanned into flame; and here he analyses his thoughts, separates them from the dross of sordid self-interest and love of the world, and presents them pure and simple before the throne of his Maker, his Redeemer and his God!"—p. 252.

There is a vast quantity of curious and original information in Chapters XI., XII., and XIII., which treat respectively of the simple combustibles, the *metalloids*, and the metals. Chapter XIV. brings us to the "electric energies of the grand movers of nature," which, with the succeeding Chapters on "geology" and "meteorology," will not admit of an intelligible abstract in the space we could here assign to it: suffice it to say, that *nebulae* and *nebulosities* are the materials resorted to for the formation not only of our globe, but of the whole planetary system, and that elec-

tricity is the agent by which the heavenly bodies are directed in their orbits. Gravitation, of course, is an effect of a subtile fluid, which "we affirm to be electricity."

The remaining Chapters of this book are assigned to vegetable and animal substances; we shall select a few quotations from them, to shew that the author remains a staunch electrician to the last. At page 564, we are told that a blight generally comes on with a blue tinge in the air, that is sometimes attended with a smell resembling that which is occasioned by an electric spark.

"May not this be carburetted hydrogen gas evolved from the earth by a charge of the negative electric fluid, and condensed into a locomotive charge, or liberated in the spring more particularly, on account of the sun advancing towards the northern hemisphere? Certainly, insects are generated in myriads after the wind has brought the blight; but then, may not the electric charges have produced a disease in the plant, or a fermentation of its juices, causing a developement and exudation of the saccharine matter, in the form of what is vulgarly known by the name of the *Honey-dew*, which is the co-operating cause of this exuberance of animal life, by affording a nidus to the myriads of ovula of various animalculæ continually floating in the air?

"Plants near the foot of thunder-rods grow vigorously; and on this principle conductors have been elevated a great height, with the design of attracting the positive fluid from the clouds and conducting it downwards to plants. These conductors were supported by non-conducting substances fixed into the ground, so that they did not come in contact with the earth or plants; but the lower part of each branched off into a great many ramifications furnished with spikes pointing downwards; consequently, the electric fluid was distributed to different parts of the garden. The result, in most cases, is said to have fully answered the expectation of the experimentalists; and to have proved, that the principles of electricity are those on which vegetation chiefly depend. The leaves of plants act as so many spicula to attract the electricity of the air and solar rays:—hence very high trees are so many natural conductors, attract a vast quantity of the electric fluid, and consequently put forth a luxuriance of foliage proportionate thereto. It is a well-known fact that positively-charged winds are colder to the leeward than to the windward of plantations or forests of high trees, by being thus deprived of their electricity.

"The ascent of the sap in the tubes of the alburnum may more reasonably be supposed to be occasioned by the electric energies, than by any inherent power in the plant similar to muscular contraction. Should a small capillary tube be placed with the lower end in water or any other liquid, the fluid substance will ascend but a very little way; but if that tube be electrified, the liquid

will immediately ascend very rapidly along it. The lower regions of the air, in winter, being mostly negative, and the solar rays having little effect, no sap therefore rises in their tubes; and the only sign of life which they exhibit, is, the protrusion of a small quantity of mucilage into the buds, which may be the last ebbings of the previous summer's sap;—and if they be carefully observed, it will be found that even this is done on those occasions when the air is positive.”—Pp. 564, 565.

These views are carried much farther, but our readers will certainly allow that we should not be justified in attempting to condense them, and we have no further space for transcription, as we must devote a page or two to the new views which are developed upon the difficult and intricate subjects of the functions of animal life. In regard, for instance, to the production of animal heat, we find that it is—

“Somewhat similar to that of vegetable heat; though the immediate sources are very different. In the latter, the sun and atmosphere furnish the positive electric fluid, and the earth the negative; so that the operation of the plant is that of replacing the electric fluids in the respective elements:—but in the former, the electric fluids are separated from their respective elements; the brain somehow or other becomes charged with the positive fluid from the carbon and hydrogen taken in as food by the stomach, while the blood is charged with the negative contained in the oxygen imbibed by the lungs. The brain branches off in nerves over all the body, and these ramifications become so attenuated, that not a particle of matter exists in the body which is not in contact, either with a large branch or some of the network of the nerves—in like manner the arterial blood loses itself in millions of invisibly small tubes called nutrients; and as the oxygen imbibed by the lungs is carried along with it by means of the red or colouring particles with which it unites, as is also the chyle recently taken in by the food, the blood is in these nutrients met by the positive electricity of the nerves, and here is the animalization carried on.”—pp. 601, 602.

Again,—

“How the brain is supplied with positive electricity is not yet clearly demonstrated, but from the great sympathy existing between it and the stomach, it seems to have its supply from the digesting food. Several arguments might be advanced in support of this hypothesis, particularly the rapidity with which alcohol, or the still more inflammable fluid, *ether*, when taken into the stomach, affects the brain. It is well known that these are the most inflammable of all substances that usually enter into the stomach, being carbon and hydrogen, in which the combustion may be said to be commenced by the small quantity of oxygen with which they are in alcohol and ether combined. Our Induc-

tion is, that as the food, or at least the nourishing part of it, is chiefly carbon and hydrogen, when operated on by the gastric juice, it parts with some of the positive fluid which ascends to the brain along the vast ramifications of nerves that embrace and perforate the coats of the stomach. Hence that rapid action of the gastric juice, sometimes called the chemical wonder; and that languor or lowness of spirits which is felt when the stomach has been any length of time empty; also that revivification on taking a little food, or more quickly so on taking a glass of cordials, which in all cases owe their virtue to the presence of alcohol. This enlivening effect is found long before such substance can be assimilated, or conveyed by the lacteals into the blood."—pp. 602, 603.

The following is too ingenious to be omitted:—

"As both arteries and nerves terminate at the skin, electricity is often, especially when the clothes worn are suitable conductors, exhibited around the human body. Any one putting off silk stockings in the dark will be sensible of this, especially if he should wear a pair of black ones over a pair of white, for they will adhere together; and if suddenly drawn off the leg or pulled asunder, there will be flashes of light. Several animals, as the cat or horse, when smartly rubbed in the dark, will emit sparks: and some people, as mentioned in the *Encyclopedia Metropolitana*, have been endowed with this very peculiar property to a wonderful degree."—pp. 605, 606.

The section on food, page 606, deserves not only to be read, but also digested. We are sorry it escaped the notice of Dr. Paris, and recommend it to his attentive perusal before he publishes the new edition of his *Essay on Diet*, which we understand is in the press; indeed, our medical readers in general will find much "new physiology" in Mr. Taylor's book: the following paragraph is an instance:—

"In almost every individual, the nerves of some parts of the body are more tender than others; and such parts will, of course, be more liable to be affected by external or different causes than others. Of whatever organs, then, the nerves may be defective, these organs will suffer the most at the approach of a storm. Should the weak nerves belong to the stomach, then indigestion, or perhaps nausea may ensue; if the lungs, asthma; if the intestines, diarrhœa, or colic; if to the ligaments of the joints, rheumatism; if to the teeth, tooth ache; if to a muscle a difficulty of motion, a contraction, or spasm, or the cramp in the calf of the leg, or a lock-jaw, and sometimes the palsy; but if the brain itself be disordered, then a violent head-ache, giddiness, delirium, or insanity, may be produced. Thus we see the same causes affect different people in various ways; but we may generally remark, that when any one is complaining of the shooting

of a corn, the throbbing of a wound, the writhing pain of rheumatism, the stupidifying heaviness of the head-ache or any other ache or pain, he generally ceases his complaint soon after the rain begins to fall, for then the electricities are discharged, and his body is freed from its oppressive load.—That invalids are not equally affected previously to every storm or fall of rain, is owing to the different modes in which the earth and air are charged. When the earth is wet and the atmosphere is cold, the lower stratum of clouds is generally charged with the negative fluid; and the positive descends from the higher regions, or comes with the clouds from distant quarters; even most thunder-storms are, in our climate, produced by the meeting of oppositely-charged clouds, with little or no discharge from the earth; consequently the nervous sensibility we are now describing must be greatly modified.”—p. 618.

Our patience is now exhausted—the mixture of metaphysical trash and theological doctrine at the end of the book, is beyond all endurance; and we cannot conclude without expressing our surprise and regret that respectable booksellers should sanction the publication of such a farrago of ignorance, untruths, and bad taste, as is contained in almost every page of this presumptuous *KEY TO THE KNOWLEDGE OF NATURE*, for we assume that the *Rev. R. Taylor* is one of those spiritual nonentities well known in the *Row*. These books are productive of more mischief than is perhaps usually suspected; they often have a large circulation in schools and country families, and thus diffuse their pernicious ignorance where it is most productive of injury.

ART. IX. ASTRONOMICAL AND NAUTICAL COLLECTIONS.

No. XXIV.

- i. *Results of Mr. de FREYCINET's Experiments on the LENGTH of the PENDULUM.* From his *Voyage autour du Monde*. 4. Paris, 1826; 5th division, *Pendule*, making half a volume.

OF Mr. de FREYCINET's *Voyage autour du Monde*, fait sur l'*Uranie*, to be contained in eight volumes quarto, the fifth division, consisting of half a volume, devoted to the observations of the pendulum, has been lately published.

Mr. de Freycinet was provided with an astronomical reckoner, or temporary clock, with the weight sliding on the rod of its pendulum: he had three or four invariable pendulums, and the place of the weight being marked by experiment for each pendulum, it was easy to obtain a perfect synchronism by some slight corrections in each experiment; and when the vibrations of the invariable pendulums became more frequent from the diminution of the arc, the place of the weight could be altered a little by the hand, *without deranging the vibration*. "*Il suffisoit de tourner avec adresse l'écrou qui supportoit la lentille, en ayant soin de ne pas arrêter les oscillations.*" (P. 7.) The reckoner was necessary as an intermediate object of comparison between the pendulum and a chronometer, and the comparison was made at the beginning of several successive minutes. The general results of the determinations are contained in the sixth chapter, p. 45.

1. The ellipticity of the southern hemisphere is not sensibly different from that of the northern.

2. It is more considerable than $\frac{1}{305}$, which is the result deduced from the lunar irregularities.

3. It may be considered as determined by the experiments here related, when separately computed for the different hemispheres, to be between $\frac{1}{810}$ and $\frac{1}{884}$.

4. The parallels of latitude are not correctly circular, as is sufficiently shown by former experiments and measurements.

5. The experiments of the Isle of France, Guam, and Mowi, compared with those of Paris, all exhibiting a smaller difference of oscillations than would be expected from theory, we must suppose the existence of considerable irregularities in these stations.

6. Omitting, therefore, these three stations, the mean of the rest affords us an ellipticity of $\frac{1}{286.2}$, which may be considered as the final result obtained from all the operations of the expedition.

It might deserve to be inquired how far this result would be modified by the consideration of the local density of the earth's surface, where the stations are elevated: our author has made his corrections as if he had been observing in a balloon, (p. 26.) It is extremely remarkable that the definitive result of these experiments should differ so little from those of our indefatigable countryman Captain Sabine; although the means employed appear to be much less capable of extreme accuracy.

Stations.	Latitude.	Length in a Vacuum.	On the Level of the Sea.
Paris	48 50 14 N.	1.00000000	1.00002271
Falkland Islands	51 35 18 S.	.99783413	.99783538
Port Jackson . .	33 51 34 S.	.99793729	.99871582
Cape of Good Hope	33 55 15 S.	.99759268	.99794215
Rio Janeiro . .	22 55 13 S.	.99792769	.99709575
Mowi I., Sandwich I.	20 52 7 N.	.99871111	.99759331
Isle of France . .	20 9 56 S.	.99876387	.99792816
I. Guam, Ladrones	13 27 51 N.	.99709528	.99877424
I. Rawak, N. Guinea	0 1 34 S.	1.00022130	1.00022319

ii. *A Letter to the Editor, from Mr. DAVID THOMSON, respecting his*
LUNAR TABLES.

Sir,

Having lately arrived from a voyage to India, I find that, during my absence, an attack has been made, in your 38th Number, on a set of Lunar and Horary Tables published by me; and as I think that your correspondent has made several unfair comparisons of my Tables with some others, I trust you will insert the following answer to his remarks.

The person who has examined my book, has evidently done so with a determination to notice what he chooses to call its imperfections, without saying a single word in its favour; and for that purpose, he has assumed a few examples of clearing the lunar distances from the effects of parallax and refraction, with the view of showing that the method given in my work is in some cases liable to error.

That such examples as those chosen by your correspondent could seldom or never occur in practice, must be obvious to every person conversant with the subject; and that my tables are not liable to any error of the least consequence, in any example that is likely to happen in practice, the following comparisons of the true distances, as found by my method, and those obtained by the usual modes of calculation, will, I trust, satisfy every unprejudiced person.

Examples taken from the second edition of Dr. Mackay's work on the Longitude, being the first six given in that work.

Moon's Hor. Par.	App. Alt. of ☉ or *	Moon's App. Alt.	App. Dist.	True Dist.	True Dist. by Lun. Tab.
58' 45"	☉ 27' 43"	48' 22"	81' 23' 38"	81' 4' 26"	81' 4' 26"
56' 32"	☉ 25' 16"	19' 19"	72' 21' 40"	72' 3' 50"	72' 3' 50"
57' 43"	☉ 35' 43"	29' 31"	38' 45' 40"	38' 28' 22"	38' 28' 21"
61' 10"	✱ 11' 51"	44' 33"	64' 36' 40"	64' 46' 14"	64' 46' 14"
59' 33"	☉ 27' 43"	46' 18"	54' 57' 35"	54' 55' 15"	54' 55' 15"
61' 16"	☉ 56' 52"	38' 24"	59' 28' 20"	58' 52' 26"	58' 52' 26"

Four Examples taken from the seventh edition of Norie's Epitome of Navigation, in the order in which they stand in that work.

Moon's Hor. Par.	App. Att. of ☉ or *	Moon's App. Alt.	App. Dist.	True Dist.	True Dist. by Lunar Table.
57 14	☉ 32 32	41 11	80 1 53	79 38 53	79 38 53
54 0	* 47 32	32 12	96 36 31	95 55 19	95 55 20
57 25	☉ 44 6	16 37	96 35 26	95 56 10	95 56 8
58 29	* 41 11	29 48	64 46 24	64 18 56	64 18 56

Six Examples from Mr. Lax's Tables.

58 25	☉ 7 38 29	46 9 28	40 36 5	41 18 54	41 18 55
54 17	☉ 48 55 49	24 43 37	86 54 25	86 16 45	86 16 44
53 59	* 28 24 59	61 36 50	33 30 21	33 56 49	33 56 49
58 36	* 36 36 39	6 10 10	30 52 15	30 1 34	30 1 36
55 2	☉ 52 38 40	31 13 50	56 4 55	55 32 42	55 32 42
58 30	* 32 5 51	43 23 33	96 3 26	95 30 4	95 30 5

Twenty-two Examples from Margett's Longitude Tables, calculated by Taylor's Logarithms, to the nearest tenth of a second.

57 57	☉ 35 34 36	34 6 17	90 12 5	89 40 4.6	89 39 5
54 58	* 34 56 51	45 39 3	41 25 52	41 23 49.8	41 23 52
54 12	* 39 15 9	53 54 15	48 40 46	48 34 39.8	48 34 41
59 24	☉ 15 27 36	29 29 43	47 11 3	47 18 13.2	47 18 12
59 24	☉ 15 47 17	29 4 18	47 8 35	47 14 53.6	47 14 52
59 25	☉ 16 6 6	28 39 59	47 6 16	47 11 49.2	47 11 48
59 25	☉ 16 48 9	27 30 39	47 0 33	47 4 7.2	47 4 6
55 51	* 46 32 9	23 19 6	32 45 51	32 6 46.0	32 6 45
55 51	* 45 8 5	21 35 6	32 40 1	32 0 17.0	32 0 16
55 51	* 43 43 8	19 54 9	32 34 57	31 54 44.8	31 54 43
55 11	* 63 27 9	13 30 35	63 37 24	62 52 4.2	62 52 5
58 16	* 51 4 56	21 58 33	51 54 20	51 15 30.8	51 15 32
58 16	* 50 23 25	23 45 18	52 3 3	51 26 0.4	51 26 1
58 41	* 49 25 29	16 12 47	65 58 10	65 19 6.0	65 19 6
58 41	* 48 34 41	17 30 36	66 3 55	65 25 50.6	65 25 50
59 22	☉ 17 43 0	11 55 56	96 17 42	96 0 23.2	96 0 23
59 24	☉ 15 36 57	17 13 7	83 12 31	83 0 12.8	83 0 14
59 24	☉ 21 30 35	14 27 51	82 55 4	82 36 49.4	82 36 50
59 24	☉ 22 15 53	14 1 13	82 52 39	82 33 41.8	82 33 40
59 1	☉ 19 3 9	23 51 57	43 28 21	43 26 48.0	43 26 48
59 1	☉ 20 25 57	24 18 12	43 24 16	43 21 10.8	43 21 11
59 1	☉ 21 27 33	24 35 30	43 21 19	43 17 8.4	43 17 6

From these examples it may be seen how very unfairly your correspondent has examined my tables. For in *none* of them

does the true distance, as computed by my method, differ so much as three seconds from the truth; and there are only four examples, out of the thirty-eight given, where the difference amounts to two seconds. In the last example given by your correspondent, to show the error to which my method is liable, he makes the error sixteen seconds, when it is only seven and a half seconds.

Your correspondent next gives an example, worked at full length by my method, with the view of showing that it is not shorter than the method given in the Appendix to the requisite tables, nor that given in Mr. Lax's tables. He, in fact, makes the latter method appear shorter than mine. This he has effected by introducing three proportions, wrought out in the most tedious manner, even to the *tenth of a second*, in order to find the third correction from my tables. I have worked many hundreds of examples by the method given in my book, and have never once had occasion to employ the *Rule of Three* in finding the third correction; nor did I ever hear of any one that had; and, certainly, a man must be very unfit to determine the longitude of a ship at sea, by the lunar method, who is incapable of making such proportions at a glance. In fact, the number of figures required in correcting a lunar distance by my tables, is only about *one third* of the number your correspondent wishes to make appear necessary.

I shall not enter into an examination of all the strange cavilling remarks that your correspondent makes on my tables, as I feel confident the object he had for making them must be obvious to most of your readers; and even if his objections were well founded,—which they are not,—they would be of little or no consequence to the practical navigator, for whose use my book is intended. I may, however, notice his concluding remark, which is as follows:—"When the parallax is very great, or very small, the correction, by Capt. Thomson's method, must confessedly be defective." Now it will be seen, by several of the foregoing examples, that even when the moon's horizontal parallax is greater than $61'$, or less than $53'$, there is not the least error in

the true distance deduced by my tables; and I have no doubt but your correspondent knows very well that it is only when the distance is very small, both altitudes low, and at the same time nearly equal, that the parallax makes the least difference in my method. That such a combination of circumstances could seldom happen is perfectly evident; but, in fact, such distances and altitudes are never observed at sea, for the purpose of determining the longitude.

22, Birchin-lane, August 26, 1826.

DAVID THOMSON.

[Note.—The “concluding remark” was added by the EDITOR.]

iii. *Account of an Astrological Manuscript, in a Letter to FRANCIS BAILY, Esq., F.R.S.*

DEAR SIR,

I have been permitted, by the kindness of Colonel Leake, on the part of the Royal Society of Literature, to insert in the Collection of Egyptian HIEROGLYPHICS, which I have undertaken to edit for that Society, a copy of a Greek manuscript, on three pages of papyrus, sent to the Society from Egypt by Mr. Salt. The fasciculus containing the plate having now been delivered to such members of the Egyptian Society, as have chosen to continue their subscriptions for the work, I think it right to address to you, Sir, as one of the best judges of astronomical chronology, some remarks on the information which may be derived from this manuscript; although, from the circumstance of the exact coincidence of the date with the age of Ptolemy, it is incapable of adding any thing very material to our knowledge of this subject.

Translation of the Manuscript.—Hierogl. 52. i.

“ May this be prosperous !

“ Having studied in many volumes the doctrines which have been handed down to us by the ancient sages, that is, by Ptolemy chiefly, and King Necheus, as they were also instructed by our Lord Hermes and Æsculapius, which is to say Imenthes, the son of Hephæstus. According to the time given to me, that is, the fourth year of Antoninus Cæsar our Lord, on the eighth of the

month of Adrian, according to the Greeks, but according to the Egyptians the eighteenth of Tybi (v.) at the first hour of the day.

“ Whence it has been found by computation, according to the least . . . in the sign Sagittarius 13 degrees . . . of the sixty . . . 35, which is a part . . . twelfth.

ii.

“ With good fortune ! The Nativity of [Anoëbion, the fourth son of Psansnos, or Psanenos] ?

“ The fourth year of Antoninus Cæsar our Lord, the 8th of the month Adrian, but according to the ancients the 18th of Tybi, the first hour of the morning.

“ The Sun in Sagittarius, 13 degrees, 23 minutes ; in the house of Jupiter, the boundaries of Venus.

“ The Moon in Aquarius, 3 degrees, 6 minutes ; oriental, in the house of Saturn, the boundaries of Mercury.

“ Saturn in Aquarius, 3 degrees 8 minutes ; in the second station, his own house, the boundaries of Mercury.

“ Jupiter in Aries, 12 degrees 44 minutes ; in the second station, the house of Mars, the elevation of the Sun, the depression of Saturn, the boundaries of Venus.

“ Mars at the end of Capricorn, 33 degrees 0 minute ; in the second station, the house of Saturn, his own elevation, the depression of Jupiter, his own boundaries.

“ Venus in Sagittarius, 9 degrees 54 minutes, near her rising, in the house of Jupiter, her own boundaries.

iii.

“ Mercury in Sagittarius, 15 degrees 2 minutes, in the ‘ west,’ in the house of Jupiter, the boundaries of Venus.

“ The Horoscope in Sagittarius, 15 degrees ; the house of Jupiter, the boundaries of Venus.

“ The Nuptial point in Gemini, 15 degrees ; the house of Mercury, the boundaries of Mercury.

“ The Midheaven in Virgo, 8 degrees ; the house of Mercury, the elevation of Mercury, the depression of Venus, the boundaries of Venus.

“ The Subterraneous point in Pisces [8] degrees ; the house of

- Jupiter, the elevation of Venus, the depression of Mercury, the boundaries of Venus.
- “ The first Lot of the Fortune in Capricorn, 19 degrees; the house of Saturn, the elevation of Mars, the depression of the Sun, the boundaries of Venus.
- “ The second Lot of the Fortune in Cancer, 11 degrees; the house of the Moon, the elevation of Jupiter, the depression of Mars, the boundaries of Venus.
- “ The Master of the Nativity, the Star of Venus.”

To this approximate translation I shall subjoin a few hasty notes.

i. *Petosiris* and *Necepsos* are quoted together by Pliny, II. xxiii. VII. xlix. as astronomers and astrologers; and *Æsculapius* is mentioned as an astrologer in the latter chapter. It is doubtful whether this “*Imenthes*” is meant for Ismendes or Osimandyas or is a name of the Egyptian *Æsculapius*, who is called by Manetho Tosorthrus, and is made a son of Pan and *Hephæstobule*.

The *fourth year of Antonine* is considered as answering to the year 141 of the Christian æra. It was in the third year of Antonine that Ptolemy observed the autumnal equinox, on the 9th of Athyr (III.), as he informs us in the beginning of his third book (P. 62. Ed. Basil.) This must have been on the 24th or 25th of September, for Ovid tells us, in the *Fasti*, that the vernal equinox happened on the 22d of March. The 1st of Athyr must therefore have been the 16th or 17th of September, and the 1st of Thoth the 18th or 19th of July; and we find, from the Article *EGYPT*, in the *Suppl. Enc. Br.*, that at the period in question the wandering year began on the 19th July. It is evident, therefore, that the year of this manuscript, as well as that of Ptolemy, in the passage mentioned, is the “ancient,” or wandering year of 365 days, and not the Alexandrian, or fixed year, of 365 $\frac{1}{4}$.

The date in the document, the 18th of *Tybi*, corresponds to a day 69 days later than the equinox of Ptolemy, and would therefore be the 3d December, rather than the 8th. On the other hand, the sun’s longitude, amounting to 8° 13′, corresponds to the 7th of December, reckoning from the same equinox. It may there-

fore be questioned, whether the month here named after the Emperor *Adrian* was meant for December, or for some Macedonian month beginning a few days earlier, though the sun's place agrees sufficiently well with the former supposition, and the Roman months had so often undergone similar changes, that the honour shown to *Adrian* may easily have been continued in the colonies for a few years after his death. This very December was called *Exuperatorius* by the Emperor *Commodus*, who had assumed a similar name among his own titles ; but all the changes introduced by this wretch were soon forgotten, as well as the names of the months which had been proposed by *Nero* and *Domitian*.

ii. The Sun is said to be in the *House* of Jupiter, and in the *Boundaries* of Venus. The houses of Jupiter, as we find from the Paraphrase of *Proclus*, (I. xx.) were the signs *Sagittarius* and *Pisces*. With respect to the boundaries, the months were variously subdivided, (Ch. xxiv.) and the divisions employed by the Egyptians and Chaldeans were somewhat different from each other. In *Sagittarius* the boundaries of Jupiter were from 1° to 12° , or to 8° ; of Venus 13° to 17° , or 9° to 14° ; of Mercury 18° to 21° , or 15° to 19° ; of Saturn 22° to 24° , or 20° to 25° ; and of Mars from 25° or 26° to 30° .

Jupiter is in Aries, the *Elevation* of the sun, and the *Depression* of Saturn, both these characters being also attributed to Aries by *Proclus*, (Ch. xxii.)

iii. The *Horoscope* appears to be the rising point of the ecliptic. (See *Proclus*. III. iii.) The *Nuptial* point is here diametrically opposite, and seems to be retiring to rest. The *Subterraneous* point is obviously passing the meridian below the earth, as the *Midheaven* is above it. Of the two *Lots of the Fortune* the one seems to be in the sign rising next after the birth, the other nearly opposite to it.

I shall not detain you with any further attempts to explain this mummary, though I suppose all the information, that could be desired, might be found in *Firmicus* and the other astrological writers.

* * * *

Park Square, 11th Sept. 1826.

T. Y.

iv. *Account of Experiments for determining the LENGTH of the Seconds PENDULUM, and the Specific Gravity of WATER.* By JÖNS SVANBERG. Translated from the Swedish Transactions, 1825. i.

It is an object of the highest importance to every civilised community, not less in a commercial than in a scientific point of view, that all the original units of their system of measures and weights should be secured, even to an ideal degree of perfection, against the gradual alterations, which must unavoidably be introduced, in the successive copying of the standards. In order to attain this object, there is no other way to be found, than by ascertaining, in the first place, by means of accurate experiments, the relations of these units, which have commonly been assumed from an arbitrary origin only, to some natural and unalterable magnitude of the same kind; and fixing, in the next place, by an express law, these proportions with absolute mathematical accuracy. With this view, and in order to preserve unaltered the Swedish ell and the victualler's pound, of the same magnitude as they have long possessed, His Majesty was graciously pleased to give orders to the Academy of Sciences, in the year 1823, to examine, with all the accuracy that the present state of science admits, the proportion of the ell to the length of the second pendulum keeping mean solar time at the observatory of Stockholm, vibrating in a vacuum, and reduced to the mean level of the sea: as well as that of the pound to the weight of a Can, that is, of 100 cubic decimal inches of distilled water, taken at a determinate temperature, and weighed in a space void of air. The management of all the requisite experiments was intrusted to myself and to Professor CRONSTRAND, with the assistance of Professor BERZELIUS in the determination of weights; and in order to do justice in all respects to the confidence of the Academy, and to contribute to the promotion of science, it became necessary, in the first place, to take proper measures for obtaining such apparatus, as was essential to the performance of the experiments, which had been graciously required of us.

Unfortunately we had no hopes of obtaining, from the artists

of our own country, such instruments as we could depend on, and we had only to choose between those of England and of France. Some of the best experimenters of both these countries had lately gone through all the steps of the proposed investigation, with the same views as our own; that is, for the purpose of determining the units of measures and of weights; and, however unbecoming it would be for us to attempt to attribute to any of the final results of these experiments a decided superiority above the rest; yet, at the same time, considering the difficulties of ascertaining with precision the elements of all the corrections which are required for the pendulum of BORDA, the advantage of easier execution may fairly be assigned to that of KATER. Such, at least, was my own opinion; and I therefore determined to apply to the English workshops for all the apparatus required for our experiments, their superior accuracy in the construction of all mathematical instruments having long been fully established. But this was not enough; it was necessary that a man of science should be found to take some interest in our proceedings, and I have reason to congratulate myself in having ventured to apply to Captain KATER, who had himself set an admirable example of accuracy in all the details of these experiments. Without having had any previous correspondence with this gentleman, and relying solely on his zeal for the improvement that might result to the physicomathematical sciences from our success, I took the liberty of addressing to him two separate requests. The first was, that he would order from the best artists in London, and that he would superintend the execution of—

(1.) A straight scale, divided into inches, or any other convenient measures of length, and comprehending the length of the simple seconds pendulum, or a little more.

(2.) A microscopic beam compass, as described by SHUCKBURGH in the *Philosophical Transactions* for 1798.

(3.) A convertible pendulum, with a moveable weight, for adjusting the centre of oscillation to a given distance from the point of suspension, as employed by Captain KATER for ascertaining the absolute length of the seconds pendulum in London, and described in the *Philosophical Transactions* for 1818.

(4.) An astronomical clock on which dependence could be placed.

(5.) A cylinder, with proper apparatus for measuring with accuracy its geometrical dimensions.

(6.) Lastly, a troy pound weight, partly in order to obtain by its means a perfect comparison of our operations on weights with those of SHUCKBURGH and KATER, and partly in order to produce the greatest possible contact between the English and Swedish metrical systems.

My other request implied somewhat more of personal difficulty to himself: it was that he would compare immediately with the English *Imperial parliamentary standard yard* the distance of 39.4 inches, on the scale which he was to procure me, and by means of which the length of our pendulum was to be ascertained; and that he would also compare the troy pound to be sent us, with the *Imperial parliamentary standard pound* [or rather the Imperial parliamentary standard *troy* pound; the distinction *troy* having been inadvertently omitted in the Act of Parliament, though it is found in the Report of the Commissioners. See No. VI. of these Collections.]

Captain KATER agreed with singular promptitude to comply with all these requests, and expressed, in a private letter, the strongest possible desire that our experiments should be completed in the most perfect and the most authentic manner.

The instruments arrived at Stockholm in the month of November, 1824, with the exception only of the troy pound; and in January, 1825, I began to take the necessary steps for the completion of the experiments: my engagements, however, as a professor at Upsal, did not permit me to conclude them till the month of May. To give an account of all the measures that we adopted, without neglecting any thing that may essentially contribute to the possibility of forming an independent opinion of the accuracy of our results, is the object of the present essay. I have, however, thought myself justifiable in wholly omitting the preliminary experiments on the inversion of the pendulum upon its separate knife edges, with the movement of the sliding weight backwards and forwards, by means of which the centre of oscilla-

tion, when it rests on one of the edges, is made to coincide with the other edge. I have also considered it as a superfluous prolixity to detail all the multiplied comparisons of Dollond's scale with the screw of the micrometer, by means of which the value of its turns was ascertained in inches. I shall only mention that we always caused the screw to act in both directions of pushing and pulling, and that the separate determinations never varied from the general mean more than .0003 inch at the utmost; which, notwithstanding the power of the microscope, may sometimes have occurred the more easily, among a great number of independent observations, as the magnitude of the error may be divided into two parts, one belonging to the initial, and the other to the final point, besides the possible want of equability in the threads of the screw itself. Such errors of observation can only be in great measure annihilated by taking the mean of a number of independent results; and I consider it as probable, that the final result of the value of the micrometrical divisions cannot be erroneous so much as .00005 inch in the whole length of the screw; and in the small extent of that part which was employed in measuring the distance of the knife edges and the dimensions of the cylinder, that the error cannot have amounted to .000003 inch. This is also true of the separate results of the last-mentioned measurements; I have therefore only inserted the means, in order to avoid filling up too much space with tabular details of separate micrometer readings, which have certainly cost the observer much time and labour, but of which the reader only wishes to know the mean result.

With regard to astronomical observations, we employed no others than those of the transits of the sun and some of the stars over the plane of the meridian mark, which were always made by Professor CRONSTRAND; and he was always careful to assure himself, at each separate time of observation, of the proper adjustment of the instrument, both with regard to its vertical motion and its azimuthal direction. The telescope has five wires, which are all employed in the usual manner, though, for the reason given above, the mean result only is here introduced, as it is corrected for the unequal distance of the wires from the middle

and for the azimuth of the mark. I must also remark, that even if there should be any uncertainty in the elements of these corrections, it will have so much the less influence on the determination of the rate of the clock, as in the course of the computation all its effect will wholly disappear, when observations on one and the same star only are compared only with each other, as I have always done in these operations.

With regard to the theoretical grounds of all the computations that are here employed, and in particular the mathematical theory of Kater's pendulum, and everything belonging to the application of the experiments made with it, I should certainly have wished to enter into a fuller detail; but the fear of introducing too tedious a discussion into an inquiry, which is rather of a practical than a theoretical nature, has prevented my inserting any investigations, which, however interesting with respect to science, may yet be properly deferred until another opportunity.

A. Transits observed by *Molineux's* clock . . . P. 7.

Both these observations of transits, and the coincidences which were from time to time tried with the detached pendulum, made it very obvious that the clock did not maintain its rate so regularly as could be wished. We thought this failure might be owing to a want of oil, and therefore supplied the deficiency, without interrupting its motion. The rate was distinctly accelerated by this addition, and a new series of observations was begun.

B. Transits observed by *Molineux's* clock . . . P. 8.

Since the same irregularities which had been before observed remained still perceptible in the rate of the clock, and since the coincidences with the detached pendulum, which were frequently repeated, continued to confirm the existence of the irregularity in an undeniable manner, we thought it necessary, in order to remove, or at least to diminish, this irregularity, to have it taken to pieces, in order to examine whether dust or any other accidental impurity was concerned in the failure. This was done in the afternoon of the 22d May, and it was replaced in the morning of the 25th. From this time a third series of observations commenced.

C. Transits observed by *Molineux's* clock . . . P. 9.

Before I attempt to deduce, from these observations, the rate of

the clock for the time of each separate series of observed coincidences; I consider it necessary to premise the following remarks :

1. We must not set out with a comparison of different stars, since the result would thus be made in great measure dependent both on the assumed difference of right ascension, and in some measure perhaps on an uncertain correction for the azimuth of the meridian mark.

2. Still less must we go from an observation of the sun to that of a star, since we should thus expose ourselves to the influence of all the imperfections that may possibly exist in our tables of the sun ; and what is of still more importance on this occasion, as I have found by long experience, during all the time that I was director of the Observatory of the Royal Academy of Sciences, to the pernicious effects of the solar rays on the pillars which support the transit instrument, which, in the course of the observation of the meridian, always occasion a perceptible deviation of the telescope from the mark to which it had just before been directed, and to which it returns in the afternoon, when the observation has been completed. These observations exhibit the same phenomenon, notwithstanding all the precautions that were employed to avoid it, and the effects are nearly constant, and always tend the same way ; so that notwithstanding their existence, the observations of the sun, compared with each other only, give for the rate of the clock results that are tolerably satisfactory. At the same time I considered it as more advantageous, for many reasons, to employ for our purpose observations of the stars only, but these were often invisible for several days together on account of clouds, and the chance of not seeing the sun was much less than the inconvenience of depending on the regularity of the clock for a longer interval, which was not justified by the observations. I have, however, always compared the observations of the sun with each other only, and employed the comparisons of the observations of stars as often as I could obtain them.

Under all these circumstances I have not thought that I could rely on any other measure of time than that which was derived from supposing the rate of the clock, derived from any two suc-

cessive observations, to belong to the instant that bisected the interval between them.

From this supposition is derived the following table, in which the number of the seconds, indicated by the clock in a mean solar day, is placed opposite to the date to which it belongs, and the acceleration of the clock in an hour is expressed in a separate column.

D. Rate of the clock...P. 11.

From this table we obtain, by means of interpolation, the following, which shows the rate of the clock for the middle of each separate series of coincidences that were observed.

E. Rate of the clock...P. 12.

Table of corrections for the observed magnitude of the arc of vibration.

[0°.50 0°.410 to 1°.85 5°.613.]...P. 13.

Coincidences observed with the greater weight

above, by SVANBERG, 1, 2...P. 14, 15.

below, by SVANBERG, 3, 4...P. 16, 17.

. . . by CRONSTRAND, 5, 6...P. 18, 19.

. 7..21...to p. 39.

Aa. Results collected and reduced to a vacuum...P. 35.

It was with great concern that we found from this comparison, notwithstanding the very satisfactory agreement between the results of each separate series of observations, that there occurred some series in which the mean result of the whole differed a complete oscillation, or more, in a day from the mean of all the series, notwithstanding that the result of each interval of the coincidences never varies more than a fraction of an oscillation from the mean of the series to which it belongs. Thus, for example, the mean of the 15th series, 86141.403, differs 1.101 osc. from the mean of the whole, 86140.302, though no one separate result of the same series differs more than .775 osc. from its mean result; and we might hence have expected, dividing the error by "the number of observations," that we might have depended on the result as true within .129 osc. [or rather .23 osc., since the divisor ought to be something more than $\sqrt{6}$, instead of e .] In the same manner the

mean of the 20th series differs 1.53 osc. from the general mean, though no single result of the separate coincidences differs more than 1.170 from the mean of the series, and the mean might therefore be supposed accurate within "one-fifth of this error, or .234" [.36 osc.]

This oscillation of the different means of the series, about the final result of the whole, may depend either on some unobserved difference between the temperature of the pendulum and that of the surrounding air, which is indicated by the thermometer, or on some unknown irregularity in the clock, which has not kept the same rate throughout the day, while the transits can only indicate the mean rate for the whole day. Thus, for example, the uncertainty of a degree [$1^{\circ}.8$ F.] in the temperature of the pendulum, occasions an error of .7738 osc. in the reduction to $+16^{\circ}.67$ [62°] and the uncertain of two degrees an error of 1.5476 osc. This is probably much more than is likely to have occurred in reality, considering the care that we always took to avoid approaching the pendulum except at the beginning and end of each series of coincidences, when it was necessary to set it in motion or to stop it, and the thermometer was always noted at the same time; but it is impossible to be answerable in all cases for the avoiding an error of half a degree, especially when the temperature of the atmosphere is rapidly rising or falling, and it is not improbable that an oscillation to the extent of .3869 osc. about the final result may be attributable to this cause.

In order to investigate the variations which may have arisen from the latter cause, that is, the irregularity of the clock, I will consider the 20th series, for which it was inferred, from the transits only, that the rate of the clock was $86402^{\circ}.03$ for a mean solar day, that is, $54^m 49^s.5$ for $54^m 49^s.4227$ of mean solar time, which was the interval between the first and second coincidence. The mean result of this series, compared with the 47.831 mean result of all the series, seems to indicate that its rate must have been $86403^{\circ}.56$, for a mean solar day, or $54^m 49^s.5$, for $54^m 49^s.3661$ of mean solar time, and consequently that, in the interval between the first and last coincidence, it must have varied $0^{\circ}.0566$ from the mean rate, deduced from the transits. This quantity, however in-

considerable it appears, is sufficient to explain the difference of 1.53 oscillations in a mean solar day, and is referrible, with undeniable certainty, to the observations of the coincidences themselves. At the same time it is absolutely impossible to obtain any knowledge of these irregularities, otherwise than by the coincidences; and it is highly probable that the positive and negative errors of a great multitude of observations, wholly independent of each other, must in great measure correct each other. It would only be by observing the coincidences throughout the day, and this for many days together, and near the solstice in a high northern latitude, that the final result would become wholly exempt from these anomalies; and though it is impossible to continue the experiment in this manner, we may at least gain a greater degree of accuracy by carrying it through as large a portion of the day as possible. We determined also that, in order to obtain a greater uniformity in the comparison of the results of the experiments with the pendulum in opposite positions, we would henceforward make all the series of observations in pairs, letting one immediately follow the other, with the pendulum inverted, the heavier weight being once above and once below, in order that the rate of the clock might be as nearly as possible the same for both positions.

22—46 observed coincidences.—P. 39-63.

General results *Bb*.—P. 64.

47—64 observed coincidences.—P. 65-82.

General results *Cc*.—P. 83.

It is inferred therefore, as the general result of all the preceding observations, that our pendulum, at $+ 16^{\circ}.67$ cent. [or 62° F.] in a vacuum, and with the great weight below, made in a solar day 86147.895 oscillations (*A*)

86148.113 . . . (*B*)

86148.068 . . . (*C*)

86148.021 mean.

And with the weight above 86146.673 . . . (*a*)

86147.421 . . . (*b*)

86147.283 . . . (*c*)

86147.127 mean.

The inequality of these two results shows that when the pendulum rested on one of the knife edges, the centre of oscillation did not exactly coincide with the other; and since it would have cost us much time and labour to adjust the moveable weight so as to produce a perfect equality between them, without any particular advantage to the final result, we considered it as better in all respects to obtain, by means of a separate experiment, a correction, x , to be added to the mean number of oscillations observed with the greater weight below, in order to obtain the true number of oscillations, N , which would have occurred if the moveable weight had been so placed as to make the times of vibration equal in both positions. For this purpose the place of the moveable weight was considerably altered, so that the number of oscillations with the great weight below, instead of being greater, became less than when the weight was above. Hence, if we call the number of oscillations already found with the weight below, M , and with the weight above, m ; and distinguish the corresponding results, after the alteration M' and m' ; we obtain the following proportion $(m' - M') + (M - m) : M' - M :: M - m : x$.

The experiments for this purpose, in two different positions of the moveable weight, were performed the 2d and 3d of June: these results are exhibited in the following tables:—

65 .. 70 . Coincidences observed (P. 86..91)

We have here $M = 86148.025,$

$m = 86147.127,$

$M - m = .898.$

On the 2d June $M' = 86200.835,$

$m = 86229.932,$

$m' - M' = 29.097.$

The 3d June $M'' = 86219.742,$

$m'' = 86258.411,$

$m'' - M'' = 38.669.$

Hence $x = 1.5180,$ or

$= 1.6276$

1.6043 mean.

Consequently $N = 86149.629.$

That is to say, that our pendulum, supposing the time of oscil-

lation with the greater weight below to have been the same as with the weight above, must have made, in a mean solar day, 86149.629 oscillations; and this is consequently the number of oscillations which would have been performed by a simple pendulum, of which the length was equal to the distance of the knife edges, and made of the same materials.—[See Note at the end. TR.]

In order to reduce the number of oscillations still further to the proposed circumstances, particularly to the level of the sea, I have assumed that the floor of the observatory is 42^m.438 above that level, as I have ascertained by a very accurate and frequently repeated levelling, in which all the requisite corrections for the figure of the earth and for terrestrial refraction, and so forth, were employed, comparing it with the actual height above the surface of the *Mälar*, on the 25th of July, 1809, which can differ but very little from the mean level of the sea.

The centre of oscillation of the pendulum, while it oscillated, was about a metre above the level of the floor, and therefore very nearly 43^m.5 above that of the sea: consequently the correction for reducing *N* to the mean level of the sea becomes = +0.58867 $\times \frac{2}{3}$ = 0.392; whence 86150.021 is the number of oscillations

which would be performed in a vacuum, and on the mean level of the sea in a mean solar day, by a simple pendulum equal in length to the distance between the knife edges, taken at the temperature of 62° F.

It remained to determine this distance by a comparison with some known measure of length. For this purpose we employed two pairs of rectangular parallelepipeds, marked AA and BB, made of quartz, on which were made two fine scratches near one of the ends, and parallel to the corresponding edge. These were pressed together by means of two springs, so that they appeared to form one substance in a single line, without a visible separation, and in this situation the distance of the scratches was measured. It was found, from the mean of a great number of observations, for the pair AA, by myself 3.0992
and by Professor CRONSTRAND 3.0984

Mean 3.0988 revolutions.

For BB, by myself . . .	2.9428
By Professor CRONSTRAND	2.9303
Mean	<u>2.9366</u>

When the distance of the knife edges was to be measured, the ends of these parallelepipeds, on which the scratches were marked, were pressed by the springs against the edges, and the distance of the scratches in that position was measured by Dollond's scale; and adding to this the distance of the scratches measured when the ends were in contact, the sum gave the true distance of the knife edges. In order to avoid the effect of any want of parallelism in the edges, the measurement was made on both sides of the bar, at equal distances from it; and it was found in this manner, according to a measurement made the 5th of May, soon after the beginning of the observations of coincidences, at the temperature of $+ 14^{\circ}.775$ [$58\frac{1}{2}^{\circ}$ F.] that the distance was

$$\begin{array}{rcl} 39.4^i - 06.245 & \text{rev. by AA;} \\ 39.4 - 0.6450 & \text{by BB.} \\ \hline 39.4 - 0.63475 & \text{mean.} \end{array}$$

The same measurement was repeated after the conclusion of the experiments; and it afforded, with the parallelepipeds AA,

$$\begin{array}{rcl} \text{The 6 June, scale at } 18^{\circ}0, & 39.4^i - 0.65725 & \text{rev.} \\ \text{8 June } 18^{\circ}3, & 39.4 - 0.64170 & \\ \hline & 39.4 - 0.64947 & \text{mean.} \end{array}$$

Besides the inconsiderable amount of the variation of this result from the former, I must remark, that even the difference which we observed is on the contrary side to what might have been expected, if the knife edges had received any injury in the course of the experiments; and this consequently adds to our confidence, besides the great care that we took in handling the apparatus, that we have no reason to fear that any such accident can have influenced our final result. The difference is indeed not greater than may be wholly attributed to unavoidable errors of observation, and gives us no reason to exclude either of the measurements from its share of influence in the mean to be adopted, which becomes therefore,

$$39.4^i - 0.64211 \text{ rev.}$$

The micrometer screw was found, by a very great number of comparisons with Dollond's scale, to measure thus :

An inch, by my observations = 102.2978 rev.

By Professor CRONSTRAND's, = 102.2905

Mean 102.2941

Consequently, a revolution makes 0.009 775 734*i*, and 0.64211*r* = 0.0062771*i*; and the distance of the knife edges, on Dollond's scale, was 39.393 7229 inches.

Captain KATER has, however, compared this scale with the *imperial parliamentary standard yard* (employed at the temperature of 62° F.) and found that 39.4 inches of Dollond are equal to 39.39931 inches of the *imperial standard measure*: we must, therefore, apply a further correction of — .00069 to reduce the distance to the actual standard measure. We have, consequently, 39.393033 inches *imperial standard measure*, for the distance of the knife edges. Hence, we have the proportion (86400)²: (86150.02)² = 39.393033 : 39.165414 inches for the length of the simple seconds pendulum in *imperial standard measure*.

It still remained to ascertain this length in our Swedish measure, and this was to be done by a comparison of the Swedish ell with DOLLOND's scale; and we employed for this purpose two standard ells, which had been made by EKSTRÖM, of equilateral four sided brass bars, one belonging to the Royal Cameral College, the other to the Royal Surveyor's Office. Although the final points of both these standards were very coarse, and somewhat scratched by the points of compasses, the middle of each of them could be distinguished with tolerable certainty. The latter gave, from the mean of five separate measurements, made by Professor CRONSTRAND,

$$23.3i + 8.0112r. = 33.378315i$$

$$23.4i - 2.2344r. = 23.378157$$

$$\text{Mean} \quad 23.378236;$$

$$\text{The former} \quad 23.3i + 7.4242r = 23.372577i$$

$$23.4i - 2.7960r = 23.372667$$

$$\text{Mean} \quad 23.372622$$

It appears, therefore, unfortunately, that these two measures

differ from each other somewhat more than is satisfactory; and as it must become questionable which of them is to be preferred, we thought it right to have recourse to EKSTRÖM's determination of the proportion between the French and Swedish measures of length, which makes the French toise = 1.0941 Swedish *Famn*, which according to KATER's comparison of the English and French standards, is equal to 1.0657648 fathom of the imperial parliamentary standard; consequently a

Fathom parl. stand. = 1.0265866 *famn*.

An inch = 0.85348883 decimal inch Swedish.

A *famn* = 0.974102 fathom, *parl. stand.*

An ell = 23.378447 inches, *parl. stand.*

The nearer approach of this result to the length of the standard belonging to the Surveyor's Office, seemed to justify us in considering this standard either as authentic, or as less erroneous than the other: we may therefore set down from the comparison with the scale,

An inch on DOLLOND's scale = 0.8554963 decimal inch.

An inch *parl. stand.* = 0.85551125 dec. inch.

A fathom *parl. stand.* = 1.0266135 *famn*.

The French *toise of Peru* = 1.0941287 *famn*.

Metre = 33.682133 decim. inches.

The length of the simple pendulum, vibrating seconds of mean solar time in a vacuum on the mean level of the sea, for the observatory of Stockholm,

Latitude 50° 20' 43" = 39.165414 inches *parl. stand.*

= 33.50645 decimal inches.

At the same time, I should consider it as too hasty, from the simple comparison of a standard, of which the complete authenticity is doubtful, to supersede the generally known and admitted proportion between the French and Swedish foot, as it was determined by EKSTRÖM: the lines of the scale being so coarse, that all possible values of the Swedish ell, between 23.378 and 23.379 inches of Dollond's scale, are perfectly compatible with them. And since it is our object, at present, not only to assure ourselves of the permanence of the ell for the future, but also to preserve it

unaltered as it has been determined by EKSTRÖM, I should think that this object would be best attained by retaining its assigned proportion to the French and English measure, and making the length of the simple mean solar seconds pendulum in a vacuum on the mean level of the sea, for the observatory of Stockholm, or $59^{\circ} 20' 34''$ N. latitude, in legal measure = 33.505574 dec. in. and in conformity to what has been already laid down

A fathom *parl. stand.* = 1.0265866 fathn.

The French *toise of Peru* = 1.0941 fathn.

Metre = 33.681256 dec. in.

Having thus determined the relation between our measures of length, and an unalterable unit of the same kind, it now remains to do the same for our weights, and to ascertain the proportion between the victualler's pound, and the weight of a kann of distilled water, taken at a given temperature, and weighed in a vacuum. This investigation, however simple it seems, supposes, however, a still more scrupulous accuracy than the former, in all the geometrical determinations; partly because we must infer the cubic contents from the linear dimensions, by means of which the influence of any error that may be committed is tripled, and partly because the linear dimensions, that must be employed, are considerably smaller than the length of the seconds pendulum; for the problem depends on the determination of the cubical content of a geometrical body, which is afterwards to be weighed both in air and in distilled water. Now since, among all geometrical bodies, the cylinder is that which can be executed with the greatest accuracy, and examined with the greatest ease, we had procured such a body from TROUGHTON in *London*, of which the diameter, without any gross error, was four inches, and the height six inches. Consequently the distance between 0 and 4, and between 0 and 6, were the parts of DOLLOND's scales which were to be employed for the definitive determination of the linear dimensions of the cylinder.

It was possible that these points might be affected by errors of division, and it was first proper to investigate whether this was the case or not. The inquiry was conducted in the following manner:—

Supposing that, in DOLLOND's scale, the distance from

$$\begin{aligned} 0 \text{ to } 6 &= x \\ 6 \text{ to } 12 &= x + er \\ 12 \text{ to } 18 &= x + e'r \\ 18 \text{ to } 24 &= x + e''r \\ 24 \text{ to } 30 &= x + e'''r \\ 30 \text{ to } 36 &= x + e''''r; \text{ then from} \\ 0 \text{ to } 36 &= 6x + (e + e' + \dots e'''')r \\ &= 6x + sr \end{aligned}$$

This distance, according to KATER's measurement, is = 35.998613 inches of the English imperial parliamentary standard yard; so that we have

$$x = 5.9997688i - \frac{1}{6}sr.$$

In the same manner, taking the distances from

$$\begin{aligned} 0 \text{ to } 4 &= z \\ 4 \text{ to } 8 &= z + \varepsilon_1 r \\ 8 \text{ to } 12 &= z + \varepsilon_2 r \\ 12 \text{ to } 16 &= z + \varepsilon_3 r \dots \\ 32 \text{ to } 36 &= z + \varepsilon_8 r; \text{ hence} \\ 0 \text{ to } 36 &= 9z + (\varepsilon_1 \dots \varepsilon_8)r \\ \text{or } 35.998613i &= 9z + \sigma r, \text{ and} \end{aligned}$$

$$z = 3.99984589i - \frac{1}{9}\sigma r.$$

The different values of e and ε have been determined with indefatigable patience by Professor CRONSTRAND, taking the mean of 50 micrometrical readings, and as many separate computations for each; thus

$$\begin{aligned} e &= -0.05742 \\ e' &= -0.03263 \\ e'' &= -0.07165 \\ e''' &= -0.07317 \\ e'''' &= -0.03071 \\ \varepsilon_1 &= -0.09563 \\ \varepsilon_2 &= -0.07326 \\ \varepsilon_3 &= -0.05650 \end{aligned}$$

$$\varepsilon_4 = - 0.09928$$

$$\varepsilon_5 = - 0.05682$$

$$\varepsilon_6 = - 0.09026$$

$$\varepsilon_7 = - 0.05600$$

$$\varepsilon_8 = - 0.05846$$

$$\varepsilon_9 = - 0.03172$$

Consequently, $s = - 0.26558$; $\frac{1}{6} s = - 0.044263$

$$\sigma = - 0.58621; \frac{1}{9} \sigma = - 0.073276;$$

and finally $x = 5.9997688 + .044263r$
 $= 6.000202$ inches *parl. stand.*
 $z = 3.9998459 + .065134r$
 $= 4.000483$ inches *parl. stand.*

We then find, by means of the coefficients e , the distances between

$$\begin{aligned} 0 \text{ and } 6 &= 6.00202i \\ 6 \text{ and } 12 &= 5.999640 \\ 12 \text{ and } 18 &= 5.999883 \\ 18 \text{ and } 24 &= 5.999501 \\ 24 \text{ and } 30 &= 5.999486 \\ 30 \text{ and } 36 &= 5.999901 \end{aligned}$$

and from the coefficients ε ...from

$$\begin{aligned} 0 \text{ to } 4 &= 4.000483 \\ 4 \text{ to } 8 &= 3.999548 \\ 8 \text{ to } 12 &= 3.999766 \\ 12 \text{ to } 16 &= 3.999930 \\ 16 \text{ to } 20 &= 3.999512 \\ 20 \text{ to } 24 &= 3.999927 \\ 24 \text{ to } 28 &= 3.999600 \\ 28 \text{ to } 32 &= 3.999911 \\ 32 \text{ to } 36 &= 3.999911 \\ 36 \text{ to } 40 &= 4.000172; \end{aligned}$$

Again, from the values of e the distances from

$$\begin{aligned} 0 \text{ to } 12 &= 11.999842 \\ 0 \text{ to } 18 &= 17.999725 \end{aligned}$$

	0 to 24 = 23.999226
	0 to 30 = 29.998712
	0 to 36 = 35.998613; and from those of ϵ ,
between	0 and 8 = 8.000031
	0 and 12 = 11.999797
	0 and 16 = 15.999727
	0 and 20 = 19.999239
	0 and 24 = 23.999166
	0 and 28 = 27.998766
	0 and 32 = 31.998701
	0 and 36 = 35.998613
	0 and 40 = 39.998785;
Lastly, from 0 to 12 = 11.999842 from e	
	11.999797 from ϵ
	11.9998195 mean
and from 0 to 24 = 23.999216 from e	
	23.999166 from ϵ
	23.999196 mean

The reciprocal agreement between these two independent determinations of the same intervals, affords a very satisfactory proof of the accuracy with which the distances from 0 to 4, and from 0 to 6 have been ascertained.

The dimensions of the cylinder were also ascertained by Professor CRONSTRAND, with the assistance of the two parallelepipeds of quartz, AA, which have been already mentioned. On each of the ends of the cylinder, which were marked No. 1, and No. 2, were drawn with black lead, four lines corresponding to each other, and at angular distances of about 45° ; and both for the measurement of the diameters and of the height, the cylinder was turned round five times, and between each pair of readings of the micrometer, for which the screw was made to push for the one, and to pull for the other, the parallelepipeds were brought into contact with it, and their even pressure against the cylinder was examined with a microscope, so that no light was visible at the point of contact. In this manner, the following results were obtained, each of which gives the mean of fifty different readings of the micrometer.

For the end No. 1.

$$Aa = 4.000483i + 1.52376r = 4.015379i$$

$$Bb = 4.000483 + 1.52218 = 4.015363$$

$$Cc = 4.000483 + 1.51752 = 4.015318$$

$$Dd = 4.000483 + 1.53220 = 4.015461$$

$$\text{Mean} = \underline{4.015380}$$

For the end No. 2.

$$Aa = 4.000483i + 1.51862r = 4.015329i$$

$$Bb = 4.000483 + 1.49760 = 4.015123$$

$$Cc = 4.000483 + 1.46634 = 4.014818$$

$$Dd = 4.000483 + 1.49772 = 4.015124$$

$$\text{Mean} = \underline{4.0150985}$$

Mean diameter of the cylinder = 4.015239 *parl. stand.*

For the height

$$AA = 6.000202i - 0.60420r = 5.994295i$$

$$BB = 6.000202 - 0.60270 = 5.994311$$

$$CC = 6.000202 - 0.58468 = 5.994486$$

$$DD = 6.000202 - 0.58596 = 5.994474$$

$$aa = 6.000202 - 0.61570 = 5.994183$$

$$bb = 6.000202 - 0.64280 = 5.993918$$

$$cc = 6.000202 - 0.62064 = 5.994135$$

$$dd = 6.000202 - 0.61642 = 5.994176$$

$$\text{Mean} = \underline{5.994247}$$

The distance of the centres of the two ends is obtained separately from each pair of diameters ; thus,

$$\text{from } Aa = 5.994239i$$

$$Bb = 5.994114$$

$$Cc = 5.994310$$

$$Dd = 5.994325$$

$$\text{Mean} = \underline{5.994247}$$

Here it may be observed, that the greatest variation in any single determination from the mean is only .000133*i*, which must be considered as a very satisfactory proof of the accuracy of the measurement.

Hence it follows that the cubic content of the cylinder was

= 75.90097 parliamentary cubic inches,

= 1.2437286 cubic decimetre,

= 47.521524 cubic decimal inches ;

and little more remained than to weigh it, both in air and in water.

For this purpose Mr. COLLIN, optician to the academy, besides a very sensible balance, which, when loaded with 2 pounds, victualler's weight, still very clearly indicated one 500000th part of the whole, had made a double set of weights, subdividing the pound decimally as far as the 100000th part. These were of brass, and were adjusted, in all their subdivisions, by Adjunct ÅKERMAN, in the university of Lund, and compared with the legal pound weight of the Royal Cameral College, which was intrusted to us for this purpose, in order that nothing might be wanting for the full authenticity of our result.

The water employed had been twice distilled, under the inspection of Professor BERZELIUS, and the apparatus was placed in the great sitting-room of the Royal Academy of Sciences, where three separate operations of weighing were performed on different days by Professor BERZELIUS, Adjunct ÅKERMAN, and myself, without any participation in each other's proceedings till the whole were completed. For the weighing both in air and in water, we had a bearer of brass made, by which the cylinder was suspended ; and since Adjunct ÅKERMAN kept the most detailed register of his proceeding, I shall begin with his account at full length.

Adjunct ÅKERMAN's weighing.

On the 21st May, between seven and ten in the morning, both the bearer and the cylinder were weighed in air, CETTI's centigrade thermometer standing at $+ 19^{\circ}.5$ in the room, and the barometer being at " 26.41," or more probably 25.91, according to the Journal of the Observatory.

The bearer with its wire weighed,

1st. 0.1224463 lb.

2dly. 0.1224471

Mean 0.1224467

The cylinder alone weighed,

1st. 2.9212915 lb.

2dly. 2.9212923

Mean 2.9212919

The 22d May, at eight, when the temperature both of the water and of the air, according to CETTI's thermometer, was $+ 16^{\circ}.9$, with the barometer at 25.8, the bearer immersed in water weighed 0.108207 lb.

The cylinder was weighed the same day, but several circumstances contributed to render the result less satisfactory; among which may be mentioned the alteration of the temperature of the water during the operation, and perhaps some want of experience in handling all the parts of the apparatus, whence may have arisen a degree of friction on the point of suspension of the balance. On this account the weighing of the cylinder in water this day is excluded from the computation.

The 23d of May the cylinder, together with the bearer, was found to weigh in water, by the first experiment, 0.109416 lb., the temperature of the water being $+ 17^{\circ}.57$; and by the second, which was made an hour later, 0.109390 lb., the temperature of the water being $+ 17^{\circ}.5$, that of the room $+ 16^{\circ}.94$, while the barometer stood at 25.36.

Of these experiments it was considered that the latter should be entitled to a double weight, since in the former the balance seemed to indicate a continual diminution in the weight of the body, so that from four o'clock till half past six in the morning, no complete equilibrium could be obtained. This shows a diminution of the temperature of the water, depending on the removal of the glass case under which the water had been kept, together with the balance, so that the vessel retained a higher temperature than that of the surrounding air for some time, and the uniformity of temperature could only be restored by slow degrees after the removal of the case. In the latter experiment, every thing seemed to be in perfect equilibrium, and the result was checked by first bringing the cylinder, with its bearer, immersed in the water, into equilibrium with the counterpoises in the other scale;

and then the cylinder with its appendages was removed, and weights were added in its place, to balance those on the opposite side, and the magnitude of these was noted. Lastly, the cylinder with its bearer was hung on once more, when the weights had been taken away, and the counterpoise was not found to differ more than 0.000004 lb., which was the excess of the latter weight of the cylinder—that is,

1st. 0.109388 lb.

2dly. 0.109392

Mean 0.109390, which was registered.

Professor BERZELIUS found,

1st, in air,

The bearer with its suspending wire 0.122447 lb.

The cylinder alone 2.921309

The thermometer + $17^{\circ}.25$, the barometer 25.46 i.

2dly, in water,

The bearer with its wire 0.1082085 lb.

The cylinder and bearer together . 0.1095150

The cylinder alone 0.0013065

The temperature of the water + $17^{\circ}.75$; the barometer 25.36.

My own experiment gave,

1st, in air,

The bearer with its wire 0.1224365 lb.

The cylinder alone 2.9212995

The thermometer + $17^{\circ}.8$; the barometer 25.375.

2dly, in water,

The bearer with its wire 0.1082090 lb.

The cylinder with the bearer and wire 0.1092275

The cylinder alone 0.0010185

The temperature of the water $17^{\circ}.0$; that of the air + $17^{\circ}.12$; the barometer 25.375.

I ought also to mention, that CETTI's thermometer was divided into quarters of centesimal degrees, and the fourth part of these divisions could be estimated with certainty; and that when it was compared with the thermometers that accompanied the apparatus, they were found to agree perfectly well together. On the

other hand, the barometer was suspected, and it was removed to the Observatory, in order to be compared with that which was daily in use there, and it was found that it stood 0.053 i. too low; so that we must add .053 to the heights of the barometer as here registered.

In order to obtain definite results from these experiments, I shall assume, that a cubic decimetre of atmospheric air, at the freezing point, in the mean state of the hygrometer, with the barometer at $^{\circ}.76$ m, weighs 1297.86 milligrammes, or 0.0030545 lb. according to Ramond's coefficient for barometrical measurements; together with Biot and Arago's immediate determinations. Hence, if B be any other height of the barometer, expressed in Swedish decimal inches, and $+ \theta^{\circ}$ be the height of the centigrade thermometer in degrees, we shall have for the weight of a cubic decimetre of atmospherical air, $B \times \frac{.0954621}{800 + 3\theta}$ lb.; and the correction for the buoyancy of the atmosphere, with respect to a certain weight P of brass, $\frac{.004716176 BP}{800 + 2.98515\theta}$; whence, since the cubic content of the cylinder was 1.2437286 dec. + .000069261 ($\theta - 16.67$) dec.; we have, for Adjunct Åkerman's experiments

The cylinder in air = 1.2439248 cub. dec.
in water 1.2437886.

The buoyancy of the bearer in water at $16^{\circ}.9$	0.0142397 lb.
the same at $+ 17^{\circ}.53$	0.0142389
Consequently the weight of the bearer in water at $+ 17^{\circ}.53$	0.1082078
But the cylinder with the bearer weighed	0.109399
and the cylinder alone	0.0011912
Correction for the buoyancy of the weights in air	0.0000002
Consequently the cylinder in water at $17^{\circ}.53$ weighed	0.0011910
the weights supposed in a vacuum.	
The weight of the cylinder in air	2.9212919 lb.
buoyancy of the atmosphere	+ 0.0033912
of the weights	- 0.0004168
weight of the cylinder in a vacuum	2.9244663

Hence the weight of 1.2437886 cub. dec. of
 distilled water at $+ 17^{\circ}.53$ 2.9232753
 Lastly, a cubic decimetre at $17^{\circ}.53$ 2.3502986

Secondly, for Professor BERZELIUS's weight,

The cylinder in air contained 1.2437790 cub. dec.
 in the water 1.2438036
 its weight in air 2.9213090 lb.
 buoyancy of the air $+ 0.0035565$
 of the weights $- 0.0004128$
 weight of the cylinder in a vacuum 2.9244527
 its weight in water at $17^{\circ}.75$ 0.0013063 in vac.
 consequently, the weight of 1.243804 dec.
 of distilled water at $17^{\circ}.75$ 2.9231464
 weight of 1 cubic decimetre at $17^{\circ}.75$ 2.350167

Thirdly, for my own experiments,

The cylinder in the air = 1.2438071 cub. decim.
 in the water 1.2437517
 weight in air 2.9212995 lb.
 buoyancy of the atmosphere $+ 0.0035385$
 buoyancy of the weights $- 0.0004107$
 weight of the cylinder in a vacuum 2.9244273
 weight in water at $+ 17^{\circ}.0$ 0.0010183

Hence the weight of 1.2437517 cub. dec. of

distilled water at $+ 17^{\circ}.0$ was 2.923409
 and the weight of 1 decimetre at $+ 17^{\circ}.0$ 2.350476

The mean of these observations gives, therefore, independently of all determinations of the expansion of water by heat, the weight of a cubic decimetre, at $17^{\circ}.428$, 2.350314 lb. And if we apply to each of them a reduction to the standard temperature of $+ 16^{\circ}.667$ deduced from Professor HALLSTRÖM's experiments on the dilatation of water, as inserted in the *Swedish Transactions* for 1823, we shall have, for the weight of a cubic decimetre at $+ 16^{\circ}.67$, [or 62° F.] according to

Adjunct ÅKERMÄN	2.350620 lb.
Professor BERZELIUS	2.350572
according to my experiments	2.350594
Mean	<u>2.350595</u>

The greatest variation of these results from the mean, though only 0.000025, [or about $\frac{1}{4}$ of a grain,] is still too great to be supposed to have arisen from any immediate error in the operation of weighing, and therefore must probably be attributed to a little uncertainty in the temperature of the water ; since it must be difficult, if not impossible, to be assured in all cases of its identity with that of the thermomter, within so small a quantity as $0^{\circ}.07$, which is sufficient to account for the greatest observed difference. From this consideration, I think it right to give the preference to no one of these determinations above the others, and I therefore adhere to the mean ; and taking, for the determination of the linear measure, the length of the pendulum 33.505574 inches, the weight of a *kann*, that is, of 100 cubic decimal inches, of distilled water at $16^{\circ}.667$, taken in a vacuum, will be 6.151951 lb.

For the comparison of this result with those which have been obtained by two so distinguished experimenters, as LEFEVRE GINEAU and SHUCKBURGH, it would have been desirable to have compared our Swedish pound immediately with the French chilio-gramme, and the English parliamentary standard troy pound ; neither of which could be done, for the want of authentic copies of those standard weights. The determination of BERCH, however, may afford, at least, an approximate comparison, making the English troy pound = 7766 $\frac{1}{8}$ aces ; while Biot makes it = 372965 milligrammes ; and finally the Swedish victualler's pound = 8848 aces. Hence employing HALLSTRÖM's values of the expansion of water by heat, we obtain for a cubic decimetre of distilled water at $16^{\circ}.667$, weighed in a vacuum,

according to LEFEVRE GINEAU,	999025 milligr.
our experiments	998819
SHUCKBURGH	998644

Had I not the most implicit confidence in the experiments both of LEFEVRE GINEAU, and of SHUCKBURGH and KATER, I should observe that our result differs only 0.00001 chiliogrammes from the mean of the three ; but it seems to be unnecessary to make any other remark, than that both BERCH's comparison of the troy pound with the aces, and Biot's estimate of its value in milli-

grammes, are founded on copies of the pound which are not well authenticated; and that, perhaps, the distance of the assumed standard temperature of $16^{\circ}667$, from that of the greatest density of water, is too great to enable us to apply the tables of HALLSTRÖM to the experiments of LEFEVRE GINEAU, with any certainty of accuracy in the last places of decimals.

At the same time, I should be the less inclined to this suspicion, from considering that if we take for the value of a French pound, *poids de marc*, a mean between BERCH's number, 10193.375 aces, and KRUSEN's, in the Hamburgh Contorist, that is, 10188, we have for its value 10190.688 aces; and consequently, as the Swedish victualler's pound is taken for 8848 aces, we have for the victualler's pound,

$$0.8682436 \text{ troy pounds,}$$

$$= 425010.4 \text{ milligrammes,}$$

and for the chiliogramme 2.352814 lb. Swedish;

whence the weight of a cubic decimeter of distilled water, in a vacuum, at $16^{\circ}67$, according to LEVEVRE GINEAU's experiments, with HALLSTRÖM's expansions, becomes 2.350589 lb., which differs from our above mentioned determination only 0.000006 lb., or $\frac{1}{5207}$ of a *lod*, or half ounce; that is, only 1 part in 391765.

Finally, I must remark that in BERCH's time it was not usual to attend to the only unobjectionable manner of comparing weights with accuracy, that is, by substituting them for each other in the same scale; and consequently that, for the satisfactory comparison of our experiments with the French and English, it is absolutely necessary that we should be provided with authentic copies of the English *Imperial parliamentary standard pound*, *troy weight*, and the French *chiliogramme*. The former, Captain KATER, with his usual readiness to oblige, promised in a letter to procure us, as soon as our application to him was made; and I am fully convinced that the men of science in France will show equal readiness in meeting the wishes of the academy with respect to the *chiliogramme*. To obtain both these objects, I have thought it my positive duty at least to express my wishes, not only for the sake of the comparison of perhaps the most satisfactory three experiments on the absolute weight of water, but still

more for the sake of obtaining the most complete contact between the metrical systems of the three nations.

From the observations which have been described, I think myself authorised to conclude, with as much confidence as can be derived from the mechanical means that were at our disposal, and from our means of employing them, that we may fix the length of the simple mean solar seconds pendulum vibrating in a vacuum, on the mean level of the sea, and for the latitude of the observatory at Stockholm, at 33.505574 decimal inches, Swedish measure; and consequently, that the space which would be described in the first second, by a body falling in a vacuum, must be 16.53434 feet.

On some other occasion I hope to be able to develop the consequences of the comparison of these observations with those which have been made in other latitudes, relative to the figure of the earth, and to "physical" astronomy in general. For the present I have thought it right to confine myself to giving an account of the experiments alone, and their application to the units of our system of measures; but I cannot avoid expressing a wish, by the way, that the same pendulum, which has now been employed for an economical purpose of the most extensive importance, should be still further applied to the solution of the no less important questions for physical science, relating to the inequalities of gravitation in considerable diversities of latitude within our Scandinavian peninsula. This inequality depends principally on the geometrical figure of the earth; and may therefore, in consequence of the multitude of experiments which have been made respecting it in other latitudes, be computed *à priori* with so little error, that it requires the utmost exertion of the accuracy of modern observations, to detect any variations from a formula which is applicable to the whole surface. At the same time these observations exhibit some anomalies which can only be explained from the geological peculiarities of the strata, which prevail differently in different places. For example, the determination here detailed differs $\frac{1}{1303}$ of a decimal inch from the result of the formula which is obtained from the French experiments com-

pared with KATER's measurement in England, and GOLDINGHAM's in Madras; and the measurement of BIOT and MATHIEU at Bordeaux differs from it almost twice and a half as much, or $\frac{1}{536}$ of a decimal inch.

These anomalies, however inconsiderable they may be, are still greater than can be ascribed to the errors which are inseparable from all observations, and are therefore connected with the physical qualities of the interior parts of the earth; so that it is always necessary to have recourse to immediate experiment for their determination, unless we choose to be satisfied with an approximate result.

With respect to the absolute weight of water, it requires the necessary determination that it should be ascertained for a precisely limited temperature; and since the very experiments, on which we have made our computations, were performed at the mean temperature of $17^{\circ}.428$ of the centigrade thermometer, it might seem that all the reductions employed in the work should be referred to the same temperature. As, however, all the operations for the pendulum had been reduced to $16^{\circ}.667$, which is the standard temperature for the scale, from which all the measures of length were deduced, I thought it right, for the sake of greater uniformity, to retain the same temperature for the operations of weighing; and in fact the difference between $17^{\circ}.428$ and $16^{\circ}.667$ is not so great, but that the influence of any uncertainty that may possibly remain in the reductions must be extremely unlikely to produce any material inaccuracy in the final result.

I have been somewhat more in doubt with respect to the propriety of making the reduction of the operations on weight to the point of the greatest density of water; but I thought the distance from $17^{\circ}.428$ to that point too great; and I should have been obliged to perform the experiments in the neighbourhood of the point of greatest density, which could not have been done conveniently, without deferring them to another time of the year. At the same time I cannot deny the real advantage and greater certainty of the results in this way of making the experiment,

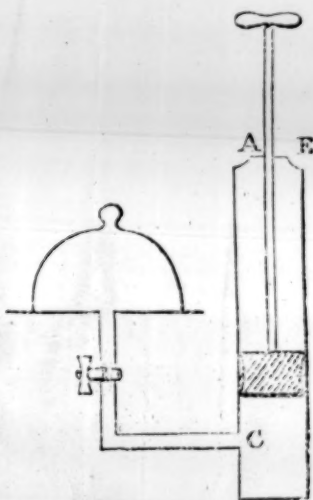
considering the difficulty, and in short the impossibility of ascertaining the real temperature of the water within a small fraction of a degree: and at the point of greatest density, the uncertainty of half a degree does not occasion a doubt of 1 in 500,000 on the whole result, while at 17° it occasions a difference of 1 in 13227. I do not indeed apprehend that there exists any such uncertainty as this, but I have thought it right to discuss what I do not consider as an absolute impossibility; and I leave it for those who are possessed of experimental knowledge, to determine within what limits they think it right to grant me their confidence. The final result of the operations of weighing is this, that a *kann* of distilled water at $16^{\circ}.667$ weighs, in a vacuum, 6.151951 Swedish pounds.

[The Note in our Next Number.]

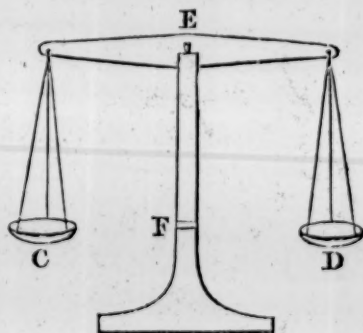
ART. X. MISCELLANEOUS INTELLIGENCE.

I. MECHANICAL SCIENCE, &c.

1. *On an Air-pump without Artificial Valves.*—The following arrangement for an air-pump, devised by Mr. Ritchie, is so simple, that we are induced to give it, for the advantage of those who are obliged to construct or repair their own apparatus. The machine consists of a barrel, shut at the lower end, and having a small aperture at C, forming a free communication with the receiver. The piston is solid, and stuffed in the usual way. The piston-rod works in a small stuffing-box at A, so as to render it completely air-tight. There is an aperture at E, in the top of the barrel, to allow the air to make its escape when the piston is raised. This air-pump may be worked in the usual way, or by the method of continued motion. In commencing the exhaustion of the receiver, the piston is supposed to be below the small aperture at C. The piston is then raised, and the air which occupied the barrel is forced out through the aperture at E; the point of one of the fingers is applied to the perforation in the same manner as in playing the German flute. The air easily passes by the finger, which, when the piston begins to descend, shuts the opening, and completely prevents the entrance of the external air. The piston is again forced down below the opening C, the air in the receiver rushes into the barrel, and is again expelled by the ascending piston. Since the air in the receiver has no valve to open by its elasticity, it is obvious that there is no limit to the degree of exhaustion, as in the common construction.—*New Edin. Phil. Jour.*



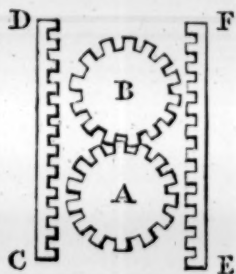
2. *Ritchie's cheap and delicate Hydrostatic Balance.*—Let a slender beam of wood be procured, about eighteen or twenty-four inches long, and tapering a little from the middle to each end. Let a fulcrum of tempered steel, resembling the blade of a pen-knife, be made to pass through the middle of the beam, a little above the centre of gravity. Similar steel blades are also made to pass through the ends of the beam for suspending the scales. The fulcrum rests on two small portions of thermometer tubes, fixed horizontally on the upright support EF. The support has a slit passing along the middle, to allow the needle at EF to play



freely between the sides. A small scale made of card, and divided into any number of equal parts, is placed at F, for the purpose of ascertaining the point at which the needle remains stationary. This balance possesses extreme delicacy. It may even be made more sensible than that belonging to the Royal Society of London.

I have said nothing of the perfect equality of the two ends, as this condition is not at all necessary to the accuracy of the balance, according to the method of double weighing. To ascertain the weight of any body, place it in one of the scales, and bring the needle to any point by means of small shot placed in the other scale; observe the point opposite to which the needle rests, or the middle between its extreme point of oscillation; remove the body, and put into the same scale as many known weights as will bring the needle to the same division as before. These weights will evidently be equal to the weight of the body, whether the arms of the balance be equal or not. This method of weighing is due to Borda.—Brewster's *Phil. Jour.* v. 118.

3. *Method of working an Air-pump by continued motion*, by Mr. Ritchie.—The method of working an air-pump by reciprocating motion is extremely inconvenient, and apt to injure the instrument by the sudden jerks to which it is liable. The following method by continued motion is free from these objections. Let there be two small wheels A B, having teeth completely round the semi-circumferences of one-half of the thickness, whilst the semi-circumference of the other half has none. The wheel A is turned by a handle in the usual way, and by the teeth in its entire circumference gives motion to the wheel B in the opposite direction. When the wheel A is turned, its teeth lay hold of those in the piston-rod C D, and raise it to its proper height. At the moment the teeth in A loose their hold in the piston-rod, those in B, moving in the opposite direction, seize those in the rod, and bring it down to its former position. The same will obviously hold true with regard to the piston-rod E F. We have thus a reciprocating motion in the two pistons, produced by the continued motion of the two wheels. The same contrivance may obviously be applied to the working of a mangle, and may perhaps answer better than the common method.—Brewster's *Phil. Jour.* v. 168.



4. *New Explosive Engine*.—Mr. Morey, of Orford, in New Hampshire, has described in Silliman's *Journal* a new explosive engine, generating a power that may be substituted for that of the steam-engine. It does not differ in principle from Mr. Brown's

gas-engine; but in place of gas, the mixed vapours of water and spirit of turpentine are used, and they are mixed with atmospheric air before they reach the cylinder in which the explosion is to take place. The vapours of alcohol and similar bodies are sometimes substituted for that of spirit of turpentine. Not the slightest data in favour of the economy or advantage of these engines above steam-engines are given; and it is remarkable, considering how important this point is in the first place, that it is so continually kept out of view in the published account of these explosive engines. There is, indeed, but little probability that it will bear investigation.

5. *Adhesion of Glue.*—Mr. Bevan has found that when two cylinders of dry ash, $1\frac{1}{2}$ inches in diameter, were glued together, and after twenty-four hours torn asunder, that 1260 lbs. were required for the purpose, and consequently that 715 lbs. were required to overcome the adhesion of one square inch of the glued surfaces. The glue was freshly made, and the season very dry. Much smaller powers were obtained with glue which had been frequently melted, the results being then from 350 to 560 lbs. Upon examining the separated surfaces of the first experiment, the glue appeared to be very thin, and did not entirely cover the wood, hence the estimation must be beneath the truth. From a subsequent experiment on solid glue, Mr. Bevan finds that the cohesion equals 4000 lbs. to the square inch, from which he infers that the application of this substance as a cement is susceptible of improvement.

The lateral cohesion of dry and seasoned Scotch fir, cut down in 1825, was 562 lbs. per square inch, and that of Memel fir, across the grain, from 540 to 840 lbs.—*Phil. Mag.* lxxviii. 111.

6. *Alteration of Oil by Filtration.*—A writer in the *Mechanics' Magazine* stated some time ago, that, upon straining some turbid, but otherwise excellent sperm oil, it, though perfectly clear and pellucid, became unfit for burning; and the Editor of the *Franklin Journal* in America says, that he has frequently experienced a similar effect. This must result from the substance through which it is strained, or from impurities in it. The former person used flannel, the latter does not mention the substance of his strainer. We understand that when filtered through clean sponge it undergoes no deterioration.

7. *Methods of Burning Lime without Kilns.*—The practice of lime-burners in Wales was formerly to burn lime in broad shallow kilns, but in some parts they now manufacture that article without any kiln at all. They place the limestone in large bodies,

which are called coaks, the stones not being broken small, as in the ordinary method, and calcine these heaps in the way used for preparing charcoal. To prevent the flame from bursting out at the top and sides of these heaps, turfs and earths are placed against them, and the aperture partially closed, and the heat is regulated and transfused through the whole mass, so that, notwithstanding the increased size of the stones, the whole becomes thoroughly calcined. As a proof of the superior advantage that lime burnt in these clamps or coaks has over that burnt in the old method, where farmers have an opportunity of taking either lime at the same price, a preference is invariably given to that burnt in heaps. This practice has long prevailed in Yorkshire and Shropshire, and is also familiar to Scotland.—*N. Mon. Mag.* xviii. 382.

8. *Improved method of making Charcoal in the large way.*—An improved method of making charcoal in the large way has been devised by Mr. Bull. It consists in surrounding the pieces of wood to be charred with pulverised coal, by which a product is afforded, equal in every respect to that made in cylinders, or retorts of iron. Besides an improvement in quality, the quantity obtained is much greater than in the ordinary method. It occurred to Mr. Bull during his experiments, that an important improvement might be made in the common process, by filling the interstices between the sticks of wood with the culm, or fine coal, left on the ground after the large coal has been drawn from the pit. In this way is prevented the access of air, which, when it takes place, not only destroys a large portion of the charcoal, but renders what remains less valuable. A charcoal-burner of New Jersey, who made some in this method, found the product to be about ten per cent. more in quantity by measure, than he had ever before obtained from the same kind and quantity of wood, and the coal when brought to market was nearly 20 per cent. heavier than usual. The coal had been well charred, and lost very little in weight by ignition in powdered charcoal. The quality was considered as superior to any other ever offered in that market; it was as cleanly to handle as anthracite coal, and sold readily at an advanced price.—*Franklin Journal*, i. 277, 358.

9. *On the employment of Fatty Bodies as a Hydrofuge in Painting on Stone or Plaster, and in rendering low and damp situations healthy.*—The memoir, of which the following is an abstract, is by MM. D'Arcet and Thenard, and contains the result of their experience on this particular subject since the year 1813. At that time the cupola of the church of St. G  n  vi  ve was to be painted, and therefore had been prepared by a coat of strong glue, followed by one of white of eggs, diffused in drying oil. Being consulted on this

preparation, the above chemists were of opinion that in time moisture would act upon it, and that the stone should rather be penetrated by a fatty body, liquefied by heat, and which, on cooling, would close all the pores. By trial it was found that a mixture of 1 part wax and 3 parts of oil, boiled with 1-10th its weight of litharge, answered perfectly well; it penetrated the stone to the depth of 0.35 to 0.55 of an inch; by cooling became solid, and in six weeks acquired considerable hardness.

It was then proposed to scrape the stone, so as to remove the previous preparation, to heat the various parts of the surface in succession by a gilder's furnace, to apply the mixture at 212° , with large brushes; as the first coat was absorbed, to apply a second until the stone would receive no more, and, to facilitate this effect, to heat the stone once or twice during its progress, according to its porosity. In all cases the heat was to be as high as possible, so as not to carbonize the oil. Finally, the walls being very dry and even, were to be covered with a coat of white of eggs, mixed with oil, on which the painting itself was to be put. All this was done, and the painting finished. At first the drops of water which, like a fine dew, nearly every morning, covered the whole interior of the cupola, caused anxiety to the painter; but they occurred again and again without producing the slightest alteration, even now, after a length of twelve years.

By this preparation not merely was the picture defended from moisture, but the absorption of the colours also prevented, and thus the necessity of varnishing the work avoided. Such was the success of the application that it was also made to other parts of the same church, where, from the extreme hardness of the stone, the depth penetrated was from 0.14 to 0.18 of an inch.

Upon extending the application to plaster, it was found to answer equally well. The process is the same, but the heat must be attended to; the plaster will bear a heat of 212° or 248° Fahr., without injury, but that of 290° would probably cause a change. Specimens thus prepared, when placed for a long time under dropping water, underwent no change in the part prepared, but were considerably roughened and corroded in the other parts.

Improvement of Apartments in low and damp situations.—The Faculty of Sciences had two apartments, the floors of which were several feet below the neighbouring houses on two sides, on which sides the walls were full of salts. They were covered with plaster some years since, but this being penetrated by the salts, became so moist as to lose its consistency, and the places became uninhabitable even in summer. A mixture of 1 part linseed oil, boiled with 1-10th its weight of litharge, and 2 parts of resin, was made, and heated until the first ebullition from the liberation of water had subsided, and all was tranquil: it was then allowed to cool. The walls were then dried, and for this purpose a gil-

der's furnace, nearly 20 inches wide and 16 high, was used ; it was easily mounted on a temporary frame, so as to be brought in opposition to any part of the wall at pleasure. In this way the walls were dried, and the work was divided, as it were, into horizontal bands, the upper ones being done first. When dry, the wall was re-heated in portions by the same furnace, and when one part had been sufficiently heated, and the furnace was removed to the neighbouring part, the former was covered with the mixture, previously brought into perfect fusion. When the absorption was not sufficiently rapid, the fire was again approached to the surface ; numerous bubbles of air were disengaged, and the absorption was quickly effected. The mixture was thus applied until the plaster absorbed no more ; five coats were imbibed, the sixth only in part, and it ultimately formed a thin varnish on the wall, of considerable hardness.

The whole surface was about 94 square metres (1012 square feet), the expense, not including labourers, 16 sous per square metre. It would be smaller upon stone, less of the preparation being absorbed. The plaster soon became very hard, and now can scarcely be scratched by the nail. In two places, where too much heated, it has been repeated. When the walls are too saline, the mixture will scarcely penetrate, in time even separates in plates, and the walls must be done anew ; the operation always succeeds on new and dry plaster.

Relative to the preparation of plaster-walls which are to be painted, the strongest conviction has been acquired, that when prepared, as the cupola was, the plaster and paintings both would be preserved in perfection. In answer to the objection which may be made, that water might penetrate from the exterior, and gradually destroy the cohesion of the plaster, it is shewn that the mixture may be made to penetrate to a great depth, rendering the plaster as hard and compact as stone, and an instance of the unchangeable and compact nature of such plaster is adduced.

Preparation of Statues and Bas-reliefs of Plaster.—As plaster thus prepared is not at all affected by water, and as by the use of soaps of iron and copper indestructible bronze tints may be given, the application of the process to works of art exposed to the weather is suggested. The substance fills all the pores of the plaster without leaving any thing upon its surface, so that the finest forms are uninjured by it, and rendered unalterable in the air. Pure linseed-oil is to be converted into neutral soap, by means of caustic soda ; a strong solution of salt is to be added, and the boiling continued until the solution is of great density and the soap floating in small grains on it : the whole is to be strained, the soap drained, and then strongly pressed, to expel the solution : it is then to be dissolved in distilled water, and filtered through a

cloth. Dissolve 4 parts sulphate of copper and 1 of sulphate of iron in distilled water, filter it, make it boil in a clean copper vessel, and add, by degrees, the soap solution, until the metallic solution is entirely decomposed; this done, add a fresh quantity of the solution of sulphates, stir and boil: the metallic soap is thus washed in the sulphates, and must then be washed in hot water and cold water, and pressed in a linen cloth as much as possible, to dry it.

Pure linseed-oil is to be boiled with one-fourth its weight of litharge in fine powder, then filtered through a cloth, and left to clear: of this 30 parts, with 16 of the metallic soap above described, and 10 of pure white wax, are to be fused in a water-bath, or by steam in an earthen vessel; the heat is to be continued until the little water present is disengaged, and then the plaster subject being heated in a stove to 176° — 194° F., is to have the fused mixture applied to it. When the plaster is so cold that the mixture will not penetrate it, it is again to be put into the stove, and the colour re-applied until as much as is necessary has been absorbed. Finally, it is to go into the stove for a few moments, that no colour may rest on its surface, and that every touch may be clear and distinct: it is then to be left in a covered place for some days, until the smell has disappeared, and finally rubbed with a fine cotton or linen cloth.

Small pieces may be dipped into the composition, shaken and wiped upon one surface, that the matter may enter at the other; but exposure of that surface to the fire has the same effect. Large pieces may be heated by the gilders' furnace. By putting shell gold on the projecting parts of the plaster, and then proceeding as described, an effect is obtained like that of the bronze appearing through the antique green surface. A larger quantity of the iron-soap produces the reddish surface of certain bronzes. Iron-soap alone gives a reddish-brown; but soaps of zinc, bismuth, and tin imitate the effect of marble; and, at times, the plaster may first be tinted by alcoholic and aqueous solutions of colouring matter, and then prepared as already described, by which numerous colours are obtained. In all cases boiled linseed-oil should be poured into the interior of the statues, to render them more impermeable to water, and diminish the consumption of the coloured preparation.

The authors of the memoir then express their conviction, that the mixture of resin or wax with the lithargerated linseed-oil may be economically employed in preserving ground-floor apartments and prisons from humidity; in rendering basins and cisterns water-tight; in preventing the transmission of water through vaults and terraces; in rendering plaster, which so easily takes every form that art can give, retentive of water; in covering statues of a soft stone, plaster medals, and many other objects, as vases,

bas-reliefs, columns, mantle-pieces, cornices, entablatures, &c.; and, finally, in preserving grain in *sylos*, or under-ground cavities.—*Ann. de Chimie*, xxxi. 24.

10. *Anomalous Case of Vision with regard to Colours.*—In the second part of the tenth volume of the *Transactions of the Royal Society of Edinburgh*, just published, is a paper on this curious and interesting subject, by Mr. Harvey. The person possessing the remarkable peculiarities of vision described in the paper is, it appears, sixty years of age, having served in early life an apprenticeship to a farmer, but disliking agricultural pursuits, became a tailor, and entering afterwards the navy, served in several general actions. After quitting the sea-service, he resumed his trade, in the employment of which he now continues. From his childhood, it appears, he was unable to point out colours by their proper names, or, excepting in a few cases, to distinguish one colour from another. From the nature of his avocation, this unfortunate circumstance must, of course, have often been to him the source of much inconvenience; and during his whole life, it appears, he has found the utmost embarrassment from it. He has remarked to Mr. Harvey, that his inability of distinguishing colours has cut him off from the enjoyment of many innocent and harmless pleasures. If a painting were placed before him, abounding with the most beautiful varieties of colour, it would only present a dull and cloudy appearance; and hence he has never made a practice of stopping at print-shops, or of visiting any scenic representations. In early life, Mr. Harvey informs us, he once visited a panoramic exhibition, and remarked that his mortification was extreme when he found every one around him delighted with the splendour of the scenes, whereas, to him, to adopt his own words, the whole presented “a smoky appearance!” The face of Nature also, which to the perfectly-organized eye presents so many exquisite varieties of colour, and so many beautiful diversities of light and shade, has always appeared to him under a dark and murky aspect. While others have contemplated, with high gratification, the splendour of the setting-sun, or the glory of the rainbow, he has seen but little to admire; and, when led by the chances of a seaman’s life into the Mediterranean, where a bright sun and a pure and cloudless sky lend to the glowing tints and the vivid colouring of Nature charms unknown to the climates of the north, the contrast produced no peculiar effect on him; nor has this arisen from a morbid constitution of mind—for, on the contrary, he is remarkably happy and chearful; and, from all the information Mr. Harvey has been able to obtain respecting him, has always been distinguished for his steadiness, chearfulness, and good conduct.

After these general observations, Mr. Harvey proceeds to illus-

trate some of the remarkable peculiarities of the case, by striking and appropriate examples.

Being desired to repair an article of dress that required *black* silk, he employed *crimson*; and a similar mistake occurred on two other occasions. On another occasion, he confounded *sky-blue* with *green*, when repairing an article of dress; and once, when a young gentleman's dark *blue* coat was brought to him for immediate repair, the mother of the lad was surprised to find the elbow of the coat repaired with *crimson*: *indigo* and *Prussian-blue* he regarded as *black*; *China* and *azure-blue* he considered to be *blue*, but thought them good matches for *carmine-red*, when placed by its side; *ultramarine-blue* he thought to be the same as *lake-red*; and when a *light lake-coloured wafer* was laid on a piece of *azure-blue cloth*, he thought the resemblance very perfect; a *blue-lilac* he called a *lead colour*; nor could he trace a shade of *purple* in the *sweet-scented violet*, or in a *plum*.

Greens were, to him, a source of much embarrassment. On a particular occasion, Mr. Harvey requested him to bring eight or ten specimens of cloth, of different shades of that colour. This, Mr. H. found, was a source of much embarrassment to him; and he was at last compelled to ask one of his fellow-workmen to point out the green bundle to him, although they had been charged not to assist him in his difficulty. His master having discovered the circumstance, substituted some pieces of *black* and *brown* for some of the *greens*, and he, unaware of the change, furnished Mr. H. with the following, as varieties of green:—

Four	specimens	of	<i>dark bottle-green</i> .
One	„		<i>reddish-black</i> .
One	„		<i>raven-black</i> .
One	„		<i>liver-brown</i> .
One	„		<i>blackish-brown</i> .

When Syme's specimen of *verdigris-green* (See Werner's Nomenclature of Colours by Syme) was placed before him, he declined giving any name to it, but remarked that it was *certainly not green*; the *beautiful green of the emerald* he called *pale orange*; *duck-green*, which forms so interesting a feature in the neck of the mallard, he named *brown or green*, displaying *much uncertainty*. All his ideas of green were extremely confused. The *darker kinds of green* he considered to be *brown*; those of a *middle tone*, *ambiguous*; and the *lighter kinds*, as in the case of *emerald-green*, of a *pale orange-colour*.

He experienced no difficulty with any of the brighter varieties of yellow; but his notions of orange were very imperfect.

Of the reds, he considered *carmine*, *lake*, and *crimson* to be *blues*; the latter, indeed, a dark blue, agreeing with the instance of the coat before alluded to. When a great snapdragon was placed before him, he called the margin of its upper lip, which

was *purplish-red*, a dark colour, and thought it a very good match for my *black coat*. The part also near the throat of the corolla, and which was of a *light bluish-red*, he called *light sky-blue*: the *yellow-palate* he distinguished perfectly; but as that colour gradually lost itself in the *purplish-red*, he gave it the name of *black*. He remarked that, when a boy, the *ruddy cheeks and arms of the milk-maidens* always appeared to him of a *bluish tone*; and, on being shown a *rosy child*, he persisted in the same remark. The carnation, pink, and the cock's-comb, presented also the same appearance. The colours of the *common garden-rose* and *peach-blossom* were both designated *lead-colour*. To him, therefore, as Mr. H. remarks, some of the sweetest and most delicate colours of creation presented but little beauty.

In the case of *browns* there was much uncertainty, in the greater variety of cases assimilating them to *green*. *Chestnut-brown*, he could not distinguish. An article of dress, indeed, of the latter colour, he repaired with silk of an *olive-green*; and, on another occasion, he considered covered buttons of a *bottle-green* as a perfect match for a dress of a *blackish-brown*. Two fragments of cloth, one a *duck-green* and the other a *liver-brown*, were placed before him, and he was unable to point out the difference.

From the whole of the observations made by Mr. Harvey on this very singular case, it appears that the only colours this person was capable of distinguishing, with certainty, by daylight, were white, yellow, and grey; and that in the proper perception of the following colours, there appeared to be varied degrees of uncertainty.

Colours Proposed for Observation.	Perceptions of the Proposed Colours.
Black.	Generally green, in particular cases crimson.
Blue.	Darker kinds, crimson and black. Those of a middle tone, carmine and lake. Those of a lighter kind able in general to distinguish.
Purple.	Appeared to be blue, excepting in the case of a very bright purple.
Green.	Confounded in general with black and brown. Greens of a darker kind appeared brown. Those of a middle tone, uncertain, as to name. Greens of a lighter kind, dark yellow.
Orange.	Darker kinds, brown. Lighter kinds, yellow.
Red.	Carmine, lake, and crimson appeared blue.
Brown.	Green.

Some time after the preceding experiments were performed, Mr. Harvey informs us he threw a vivid and well-formed solar spectrum on a white wall. The person with the anomalous vision pronounced it to be composed of two colours, yellow and light blue; and which, in a former conversation, he described as the ordinary appearance of the rainbow. The vivid and brilliant red of the spectrum he could by no means distinguish: its general appearance he regarded as, in some degree, beautiful; but the bursts of pleasure which escaped from Mr. Harvey's children, as they contemplated the brilliant colours in succession, excited in the subject of the paper the greatest surprise. Mr. H. afterwards found, that he regarded the prism as a thing moderately beautiful, but as by no means meriting the praises that had been bestowed on it.

Of the hypotheses that have been proposed to account for the phenomena in question, and many have been proposed, that which refers it to an insensibility of the retina to certain impressions of light, Mr. H. regards as the most simple and philosophical. Some eyes, observes Mr. H., are capable of performing *the general functions of vision, and are yet unable to perceive those minute impressions of light which, to other eyes, are readily perceptible*. A retina may be perfectly adapted to receive the due and proper effect of a beam of light, and yet, from some peculiarity in its organization, incapable of perceiving all its component parts. In the present case, Mr. H. observes, the general functions of vision, as relating to *form, distance, and magnitude*, were perfectly fulfilled; and the sensations arising from white, yellow, grey, and the lighter varieties of blue, appeared in general to be correct; and it hence appears probable, that the retina was sufficiently sensible to receive correct impressions from pure white, up to colours of a certain intensity, and beyond which, its power being enfeebled, it communicated only imperfect and confused ideas.

In some cases, this peculiarity of vision appears to be transmitted through a son, without in the smallest degree affecting a daughter; and, in other cases, precisely the reverse. In the present instance, however, the anomaly appears to have originated with the individual, no member of his family being similarly circumstanced, he having always regarded himself as a singular and unfortunate exception to the whole.

Since the above paper was read before the *Royal Society of Edinburgh*, Mr. Harvey has published, in the ninth number of the *Edinburgh Journal of Science*, a paper on the "Effects of Time in modifying anomalous Cases of Vision." In this paper it is observed, that with respect to the general decay of vision, which *time* commonly produces in eyes in every respect perfectly organized, it may be remarked that those perceptions of colour which are active and perfect in youth, are commonly preserved through life, with no other change than that general diminution of their

vividness and intensity, which the natural decay of the energies of the retina may be supposed to produce. But we are by no means certain, that *time* produces in eyes, imperfectly constituted in these interesting relations, analogous changes; whether age, in depriving them of the enjoyment of the minuter impressions of light, contributes to alter or modify their perceptions of colour, in any other way than a general diminution of its brilliance and power. This, it may be said, can only be determined by correct observations made at different periods of life; and perhaps Mr. Dalton is the only philosopher capable of affording any *experimental* information on the subject, since, in early life, he examined particularly into the peculiarities attendant on his own remarkable perceptions of colour, and, no doubt, has continued to observe, with that cool and discriminating attention for which he is so remarkable, whatever changes may have taken place in his vision, during his useful and brilliant career.

Time, continues Mr. H., is an element too often neglected in our philosophical investigations; and we are apt sometimes to abandon an inquiry, when the materials for its prosecution are only to be obtained by observations made at distant intervals of life.

II. CHEMICAL SCIENCE.

1. *Delicate Capillary Thermometer*.—We described in the seventh volume of this journal, at p. 183, the delicate thermometers constructed by Landriani; on some of them, a degree of Fahrenheit would occupy five or six inches. We now notice those made by Mr. Fox, which are constructed in a different way to those of Landriani's, and may at times be found of considerable advantage. Each degree on them is more than an inch in extent; and the instrument is so delicate, that eight drops of water, nearly boiling, being put into a pint of water at 60°, the quicksilver rose nearly the eighth of an inch. The method pursued in the construction was as follows:—A bulb, or ball, was blown at one end of a common thermometer-tube; this was filled in the usual way by applying heat to the bulb, on which the atmosphere forced the mercury into the partial vacuum within the ball. This done, the tube was made red hot in the flame of the blow-pipe, and drawn out into a fine capillary tube; this may be finer or longer according to the intended delicacy of the instrument. During this operation the capillary tube contained no mercury, which remained in the lower part of the original thermometer-tube, occupying also the whole of the bulb. A small piece of writing-paper was then tied round the extremity of the tube, so as to form a cavity, into which a little mercury was poured. The tube thus prepared was suspended by the upper end with the fingers; and the mercury in the ball, being

very gradually expanded by heat, in a short time rose up and filled the whole tube, until it came in contact with the mercury in the paper cavity at the top. Then, the instrument being allowed to cool, the mercury passed again into the minute tube, and by cohesion drew an additional portion after it out of the paper cavity, until the whole tube was filled at the common temperature of the air of the room. The bulb should be placed in water of the temperature to which the thermometer is desired to rise, which will cause the superfluous quicksilver to flow out at the top. The capillary-tube may then be hermetically sealed, and fixed on a graduated scale.—*Phil. Mag.* lxxviii. 71.

2. *On the Gradual Depression of Mercury in Thermometers.*—Relative to the changes that are stated to have been observed in thermometers, consisting in a slight depression of the mercury after a great lapse of time, and which have generally been referred to a permanent enlargement of the bulb, produced insensibly during the successive heatings and coolings to which it has been exposed, Mr. Blackadder remarks, that probably they may be occasioned by the presence of air in the instrument. It is known to be exceedingly difficult to exclude all the air from these instruments, in consequence of the tenacity with which it adheres to glass surfaces—such perfect exclusion, indeed, never being attained in the ordinary instruments. If then it be supposed that some air, more or less, has been left in the bulb and stem of the instrument, in the course of years this air will be decomposed by the mercury, the oxygen at least will be absorbed, and in becoming solid will have its bulk greatly diminished. In this way, the lowering of the mercury from its original height will be accounted for; but even before the included air has been decomposed by the mercury, some of it that has adhered to the inner surface of the bulb and that part of the stem that was not left empty, will escape to the upper part of the tube, and thus occasion a slight difference in the height of the fluid. The most obvious and certain preventive of such an effect would be, as Mr. Blackadder suggests, to allow a considerable space of time to elapse between the construction and graduation of the instrument.—*Brewster's Phil. Jour.* v. 47.

3. *On Magnetic Phenomena occasioned by Motion.*—The beautiful and now well-known experiment of M. Arago has been repeated everywhere with full success; but his results with substances *not metallic* having been doubted by MM. Nobili and Bacelli, that philosopher has detailed the results of some experiments, in the *Annales de Chimie*, xxii. 213, which fully confirm his first statements. He says, "I suspend a magnetic needle horizontally over water, and move it 53° from its natural position;

then, left to itself, it oscillates on each side of the magnetic meridian in arcs becoming smaller and smaller. I watch for the moment when the semi-amplitude is only 43° , and count how many oscillations have taken place from the commencement. When the distance between the lower face of the needle and the water was 0.0256 of an inch, it lost 10° in thirty oscillations; but when it was 2.055 inches, it required sixty oscillations for the same loss.

"One cannot be deceived when such a difference exists; and I may add, it would have been greater, if the amplitude at first had been 90° instead of 53° . The following are the results given by the same needle when placed over ice:—

"From 53° to 43° at 0.02756 of an inch distance, 26 oscillations.

"	at 0.04961	"	34	"
"	at 1.20081	"	56	"
"	at 2.95516	"	60	"

"On a plate of crown glass with another needle:—

"From 90° to 41° at 0.03583 of an inch distance, 122 oscillations.

"	at 0.03898	"	180	"
"	at 0.11969	"	208	"
"	at 0.15788	"	220	"

"Thus, far from the magnetic effects of non-metallic substances, as water, ice, glass, &c., being inappreciable, as has erroneously been announced by MM. Nobili and Bacelli, we see that they have sufficient intensity to induce a hope, that even the action of condensed gases may be rendered sensible when the experiment is made with proper care."

After some further observations in reply to MM. Nobili and Bacelli, M. Arago proceeds to remark on the general explanation of the phenomenon which has been given by all who have endeavoured to account for it, and to point out its insufficiency. "All the philosophers," he observes, "who have been occupied by the phenomena arising from the magnetism of bodies in motion, explain them nearly in the same manner. If a needle, say they, is horizontally suspended over a metallic plate of indefinite extent, there should be found under each pole of the needle, under the north pole for instance, a pole of the contrary name, or an attractive pole arising from the decomposition of the neutral fluid in the plate. When the plate is rotated, the attractive pole is drawn in the same direction as the rotation; a new pole is formed under the needle, which is carried away in turn, and so on. Suppose that these poles are occasioned instantaneously, and that they require some time for their disappearance, and then the needle will be preceded by a series of attractive poles, which will deviate it from its ordinary position in the direction of the motion of the plate.

"This explanation suggested itself to me when, for the first time, I communicated the experiments of rotation to the Aca-

demy, but I made no mention of it: an hypothesis, which accounted for nothing more than the *direction* in which the needle was displaced, did not appear to me as resting on a foundation of sufficient solidity. That which an hypothesis ought to prove, in my opinion, is why a copper-plate, which, in a state of rest, can hardly deviate a magnetic needle a single second, should at the same distance, by the single circumstance of its being in motion, be able to draw it to 90° and more; I freely avow that I have found no such proof. Now I have reason to congratulate myself on my reserve; new trials have shewn me that the hypothesis in question is not merely insufficient, but also directly *contrary* to the results of experiment: which is thus shewn in few words:—

“The south pole which the north pole sows, so to speak, according to the theory of MM. Herschell, Babbage, Nobili, Prevost, &c., on the edge of the moving copper-plate, should evidently, by their combined action, draw the north pole, and tend to make it approach the plate; but I have assured myself, on the contrary, that the resultant perpendicular to the plate of all the forces to which its motion gives birth, is a *repulsive force*! Suspend a long magnet by a thread, in a vertical position to the beam of a balance, and counterpoise it by weights on the opposite side: if the plate be then turned under the magnet the equilibrium will no longer exist, the magnet will appear to become lighter, and will rise—in fact, the plate will *repel* it.

“The experiment is more easily made with the dipping-needle. When the plane of such a needle is directed towards the centre of the turning disc, which I always suppose horizontal, if the needle be horizontal, all rotatory motion about the axis, passing through it, must evidently result from a force perpendicular to the disc; now, if we suppose that only one of the poles of the needle corresponds vertically with the plate, we shall find, as in the experiment of the vertically-suspended magnet, that during the rotatory motion this pole is constantly raised.

“The action exerted by a circular horizontal disc turning upon its centre, upon one of the poles of a magnetic needle, may be decomposed into three forces: the first, vertical or perpendicular to the disc; the second, horizontal and perpendicular to the vertical plane, which contains the ray or line abutting the projection or pole of the needle; the third, directed parallel to this same ray. The first is repulsive, as has just been seen; the second is the tangential force which gives the rotatory movement to horizontal needles; the properties of the third may be studied by using a dipping-needle placed vertically, and so that its axis of rotation shall be contained in a plane perpendicular to one of the radii of the disc: in this position the needle will move only by virtue of the force directed towards the centre.

“Conceive that such a needle corresponds vertically with the

centre of the turning disc, the rotatory needle will of course cause no deviation of it. There exists another point nearer to the edge than the centre, in which the verticality of the needle is also preserved. Between these two points the *inferior* pole is constantly attracted towards the centre, whatever be the rapidity of the rotation; farther out, it is *repelled*. The action is still sensible and repulsive, when the prolonged vertical direction of the needle is beyond the circular contour of the disc: I might inquire how it is that this repulsive force, directed in the line of the ray, is deduced from the action of attractive poles distributed on the upper surface of the metal, if I had not already proved the insufficiency of that theory, by the single fact of the existence of a repulsive force perpendicular to the turning disc."

In a note, M. Arago says, "I shall insert in a detailed memoir, which I am preparing, the simple experiments from which I have deduced the ratios of intensity of these different forces, for various distances of the pole of a needle, from the surface of a plate and its centre of rotation, as well as for various plates; all I wish at present is, to shew the use which I have for a long time made of the dipping-needle in the study of these singular phenomena, and the results drawn from it."—*Annales de Chimie*, xxxii. 213.

4. *On the Magnetic Influence of the Sun*, by Professor Prevost.—Multiplied observations, both ancient and modern, appear to prove that the southern hemisphere of the earth is colder than the northern. This fact is explained on the theory of radiant heat, and is no otherwise explicable. The following is, in few words, the state of the subject, and the foundation of the assertion just made. The southern winter is longer than the northern winter, its summer shorter. But this circumstance occasions no difference in the quantity of solar radiation to the two hemispheres in the course of the similar seasons. It may be demonstrated that, in the elliptic orbit, the difference of the distance of the sun compensates exactly the effect of the duration of the corresponding seasons; the quantity of rays received by the earth is constantly the same for an equal number of degrees of the ecliptic through which the sun has passed, or, in other words, the quantity of heat received by the earth from the sun is proportional to the true irregularity. The earth receives the same quantity of solar rays from one equinox to the other. So that for a season of the same name, a summer for instance, each of the two hemispheres, similarly situated with respect to the sun, receives exactly the same half-yearly illumination, measured by 180° of the ecliptic. If, therefore, one of these hemispheres is more heated by solar radiation than the other, it cannot depend upon the quantity of rays which it has received; it therefore remains to be observed whether it may depend upon the different manner in which the dis-

tribution of this quantity is made. But if we consider all this quantity as fixed, and, as it were, stored up in the earth, any inequality in the mode of its distribution would not be of much consequence; and therefore it may be said generally, that the inequality in temperature of the two hemispheres does not immediately depend upon the heat they receive from the sun.

But we know that all the rays thus received do not remain imprisoned in the terrestrial globe. One portion emanates by radiation, and is lost in space. If, according to the theory of radiation, we attend to the different distribution of the solar heat in the two terrestrial hemispheres; if, for example, confining ourselves to the summer (of which the influence is greatly predominant), we consider closely the effect which the length of the northern summer has over the radiation, we shall find that the southern hemisphere is most cooled, and that, after deducting the effect of radiation, there will remain a superiority of heat for the northern hemisphere. This is the result of a course of reasoning and calculation which I need here only refer to*.

These truths, which I published in 1792 and 1809, have been powerfully confirmed by the experiments of M. de la Roche, and MM. Dulong and Petit, if we may admit that that which takes place at very high temperatures is applicable at lower degrees. But I will not stop at this remark, nor at others which relate to heat, having another object in view.

That which has been demonstrated, of the heat emanating from the sun, is applicable, without difficulty, to every other emanation proceeding from the same source. Proceeding on the supposition already generally received, of two magnetic fluids, I think (in my conceptions of the nature of these fluids) that the magnetism of the terrestrial globe may arise from the abundance, or excess, of one of them on one of our hemispheres. Then viewing as probable the emanation of such a fluid from the sun, or that in some manner the solar emanation affects magnetism, I propose to examine whether the abundance in excess of this fluid on our hemisphere, may not be attributed to the same cause as that which produces an excess of heat. Finally, I will remark, that if these suppositions are verified, we may hope to find ratios very necessary to be observed between magnetic variations and the known movements of the axis of the earth. These thoughts have occurred to me in consequence of an experiment, which seemed to indicate the emission of one of the magnetic elements from the sun †.

5. On Mariotte's Law of the Rate of Compression of Gases.—Pro-

* *Calorique Rayonnant*, (Paris, chez Paschoud, 1809) § 285, &c.

† This experiment is that of Morrichini, repeated and varied by Mrs. Somerville.

fessor Oersted has published a series of experiments, made apparently with great accuracy, relative to the truth of Mariotte's law, as it is called, of the volume of a gas, when compressed, being inversely as the pressure, and has obtained results fully confirmatory of it. The pressures, up to eight atmospheres, were obtained and measured by a column of mercury, and present very slight deviations from the law. They were continued up to 110 atmospheres in another form of apparatus, and gave equally decided results.

To ascertain whether any difference existed, dependant upon approximation to the point of liquefaction, common air and sulphurous acid were experimented with at one time. The result was, that both volumes were diminished in a constantly equal ratio, till the moment when the sulphurous acid gas began to liquefy. These experiments, M. Oersted says, will suffice to prove, that the compression of atmospheric air and of gases is in proportion to the compressing forces, however great the latter may be, supposing that the gases remain in their aëriform condition, and that the caloric liberated by the compression has been carried off. The compression of liquid bodies reducible to drops is, as far as our experience yet goes, subject to the *same* law. Here, too, the compression and the compressing power seem to bear a direct relative proportion. We may therefore assume that the gases converted into liquids reducible to drops, begin again to follow the same law to which they answered as gases. If this should be confirmed by further experiments, it may be said that the compression of a body ceases to conform to these rules only in its moment of transition from one state of aggregation to another.—*Phil. Mag.* lxxviii. 102.

6. *On the Electricity of Gases, and the Causes of Atmospheric Electricity.*—M. Pouillet, in a memoir on the above subject, deduces that no electricity is developed by fusion, or vaporization, or by the inverse changes in the state of bodies. That during combustion, combustible bodies assume the negative state, and the supporters of combustion the positive state, and that the electricities pass from the combining molecules to those which are ready to combine. That as soon as the vegetation of a plant is well developed, it produces electricity in the earth. That alkaline solutions of soda, potash, baryta, and strontia, however little they be concentrated, give electricity; the alkali which remains after the evaporation of the water is positively electrified. The other solutions of salts or acids equally yield electricity, and the bodies combined with the water then acquire negative electricity.—*Annales des Mines*, xii. 131.

7. *Relation of the forms of Crystals to their dilatation by Heat*, by

M. Mitscherlich.—We have already noticed the curious results obtained by M. Mitscherlich *, as to the dilatation of carbonate of lime by heat. The following are the more general consequences at which he has arrived.

Crystals which possess only simple refraction, dilate equally in all directions : the action of heat does not alter their angles.

Those crystals whose primitive form is either a rhomboid or a regular hexahedral prism, are affected in a transverse direction very differently to what they are in the direction of the principal axis ; the three axes perpendicular to the latter dilate equally.

Crystals whose primitive form is a regular or rhomboidal octahedron, and, in general, all those which have two axes of double refraction, dilate differently in their three directions, the smaller axes dilating more in proportion than the larger.—*Ann. de Chimie*, xxxii. 111.

8. *Variation of the Primitive Forms of Salts.*—We are informed that a German chemist, Dr. Wöllner, has found that one and the same salt assumes different fundamental or *primitive* forms, according to the nature of the liquor in which the crystals are found. In illustration of this statement he says, that when a small portion of solution of sulphate of iron is poured into a solution of alum, and the whole allowed to crystallize, the sulphate of iron assumes the octahedral form of the alum, although these octahedral crystals contain scarcely a trace of alum.—*Edin. New Phil. Jour.* i. 189.

9. *On the mutual Re-action and Aggregation of Solid Particles.*—Dr. Fitton, when speaking of certain geological aggregations from New Holland, quotes, in a note, the effect which sometimes takes place, in the mixture of which the ordinary earthenware is manufactured, where, though the ingredients are divided by mechanical attrition only, a sort of chemical action produces, under certain circumstances, a new arrangement of parts. He says, “the clay and pulverised flints are combined for the use of the potter, by being first separately diffused in water, to the consistence of thick cream, and, when mixed in due proportion, are reduced to a proper consistence by evaporation. During this process, if the evaporation be not rapid and immediate, or if the ingredients are left to act on each other even for twenty-four hours, the flinty particles unite into sandy grains, and the mass becomes unfit for the purposes of the manufacturer. I am indebted for this interesting fact, which I believe is well known in some of the potteries, to my friend Mr. A. Aikin. And Mr. Herschell informs me that a similar change takes place in recently-precipitated car-

* See Vol. XVII. p. 157, of this Journal.

bonate of copper, which, if left long moist, concretes into hard gritty grains, of a green colour, much more difficultly soluble in ammonia than the original precipitate.—*Phil. Mag.* lxxviii. 33.

10. *On the Ignition of Gunpowder by the Electric Discharge.*—That gunpowder may sometimes be ignited by the electric discharge, but that generally it cannot be so inflamed, is very well known to all electricians, and the efficacy of water and other fluids interposed in the circuit of discharge, has frequently been pointed out*. Mr. Sturgeon, of Woolwich, has lately examined this effect, as it relates to the advantageous use of water, with some attention, and has very much facilitated the arrangements by which it may be obtained.

On repeating the experiment with water, he could not fire the gunpowder, having apparently used too large a mass of the fluid; but he observed that the force of the shock was considerably abated, the gunpowder not much scattered about, and the report trifling to that occasioned by a discharge through metal only; but he was not able at first to determine whether these effects were due to a retardation, or to some change in the physical character of the electric fluid.

Some time afterwards it occurred to him, that, if due to retardation, it might depend upon the inferior conducting power of water, and if so, that the velocity of the electric fluid might be reduced to almost any degree, by reducing the diameter of the column of water through which it had to pass. Not having any narrow tubes at hand to verify this idea, he resorted to non-conducting substances, moistened with water. A thread of sewing silk, about four inches long, was well moistened, by being drawn between the lips, and made part of the circuit between the inside and outside of a charged jar; at another part of the circuit an interruption was made between the extremities of two wires, and some gunpowder placed there. On discharging the jar, the gunpowder was ignited. The experiment was repeated several times with the same success. It was then varied by using the same thread and a smaller jar, and the gunpowder ignited with about thirty inches of charged surface. When the thread was very wet, Mr. Sturgeon never succeeded with the small jar, owing, as he supposed, to the quantity of water contained in the thread being too great to retard sufficiently the small quantity of electric fluid contained in the jar, for by squeezing out some of the moisture the thread became a worse conductor, and thus he always succeeded.

The next trial was to ascertain how far it was possible to aug-

* See Vol. XI. p. 391, of this Journal, &c. Mr. Sturgeon has evidently been unacquainted with this communication, for he states that no author has given the dimensions of the water employed.

ment the quantity of water, and still succeed with the first jar. For this purpose a piece of twine was used, well soaked in water. This twine, however, conducted the electric fluid with too much facility to ignite the gunpowder; but when some of the moisture was squeezed out, it answered very well. Thus, by proportioning the one to the other, success was always obtained. The same results were obtained by using moistened paper.

From these, with other experiments, Mr. Sturgeon concluded that the ignition of gunpowder by the electric fluid depends upon the time occupied by the latter in passing through it, and not on any change in the physical character of the fluid. It was observed in these experiments, that the moistened thread soon became dry, and Mr. Sturgeon thinks rather from decomposition than evaporation. No kind of proof is given for this opinion.

Mr. Sturgeon says, "I have frequently discharged a jar through my own body, without any other inconvenience than a burning sensation at the extremities of the fingers, and have ignited gunpowder in the same circuit. So modified is the electrical discharge, by being transmitted through aqueous conductors, that the effect of an intense charge of the most powerful battery may be reduced to almost any degree. I have discharged eight feet of charged surface* through my own body, without feeling the least shock. But the burning sensation was very severe."—*Phil. Mag.* lxxvii. 445.

11. *On the property possessed by certain Metallic Powders of inflaming spontaneously in the Air*, by M. G. Magnus.—When the pure oxides of iron, cobalt, or nickel are reduced by hydrogen gas at temperatures but very little above that of boiling mercury, metals are obtained, which, when allowed to cool in the hydrogen gas, inflame spontaneously by exposure to the atmosphere. If the reduction has been effected at a red heat, this does not take place.

When the same oxides are mixed with a little alumine, the metals obtained as before inflame spontaneously in the atmosphere, even though the heat used has been that of redness, and yet from the quantity of oxygen disengaged, it has been evident that the alumine has not been de-oxidized.

When a metal, thus competent to inflame in the air, is heated in carbonic acid gas, it loses its peculiar property, but re-assumes it upon being re-heated in hydrogen gas, and allowed to cool as before.

Nevertheless, the hydrogen is not the cause of this inflammability; for when oxalate of iron is heated in a vessel with a narrow neck, so that the acid may be decomposed and the whole allowed to cool, the metallic iron-powder obtained inflames spontaneously in the atmosphere. No other metal but the three mentioned have presented this phenomena.

* Eight feet of lining, and eight of coating.

It results from these experiments, that when the difficultly-fusible metals are in a state of extreme division, and have not aggregated either from adhesion or softness, they have the property of inflaming in the air. This effect is probably due to a power possessed by these metals of condensing such quantity of oxygen on their extended surface, as to occasion the conditions necessary to the oxidation of the metal.

The inflammability of Homberg's pyrophorus, prepared by heating to redness a mixture of alum and flour, depends probably on the same cause; for the inflammation does not take place, except when the heat has been so moderated as to be insufficient for the fusion of the sulphuret of potassium.

These phenomena are analogous to those observed by M. Doberiner, as belonging to platina, and with the faculty which silicium and zirconium have of oxidating under certain circumstances, as M. Berzelius has shewn. Perhaps, also, they may assist in discovering the causes of the formation of nitric acid in artificial nitre-beds.—*Annales des Mines*, xii. 210.

With these effects should be ranged the remarkable one observed by Dr. Gobel, as produced by the residue left upon igniting the tartrate of lead in close vessels. See Vol. XVI. p. 385, of this Journal.—Ed.

12. *Substances that continue the Incandescence of a Coil of Platina wire held over, or in their vapour.*—Mr. Miller states, that the substances discovered by Sir H. Davy*, known to produce this effect, were camphor, alcohol, spirits of wine, sulphuric ether, phosphorized ether, nitrous ether—to which should have been added hydrogen, carburetted hydrogen, and inflammable gas, when with a proper admixture of air.

By a numerous series of experiments, Mr. Miller has ascertained that the following bodies produce the same effect: benzoic acid, oil of turpentine, oil of ether, spirits of camphor, the essential oils of amber, anniseed, carraway, cinnamon, cloves, juniper, lavender, origanum, nutmeg, savine, rosemary, and peppermint. Gaseous mixtures of hydrogen, carburetted hydrogen, and olefiant gas with oxygen and air, are also inserted.

"All substances," Mr. M. observes, "whether solid, liquid, or gaseous, containing oxygen, hydrogen, and carbon in their composition (the two former are the only essential, though the latter is one of the constituents of most of the bodies experimented on), provided they are volatile either at the temperature of the atmosphere, or by the application of heat not sufficient to decompose them, seem to produce this effect.—*Ann. Phil. N. S.* xii. 21.

13. *On the Precipitation of a Metal from Solution by other Metals,*

* Philosophical Transactions, 1817.

by Professor Fischer, of *Breslau*.—The reduction of a metallic oxide from its solution in an acid or alkali by a metal, depends upon the following causes:—

1. Principally the relative affinity of the two metals for oxygen.
2. The affinity of the oxide of the reducing metal for the acid, or alkali:—for this reason tin, bismuth, and iron only reduce a small number of salts, whilst zinc reduces a great number. It is for the same reason that there are but few alkaline solutions reduced by metals: silver dissolved in ammonia is not reduced by zinc; copper dissolved in ammonia is not reduced by tin, antimony, bismuth, or iron.
3. The electric tension which may exist between the precipitating and precipitated metal.
4. The affinity of the metals for each other:—copper is precipitated by iron in a state of purity; precipitated by zinc, a kind of brass is formed.
5. The state of saturation and concentration of the solution:—the precipitation is more rapid as the fluid is more acid and concentrated. If the metal is required in fine dendritical crystals, the solution should be dilute. Many metallic salts, reducible by certain metals when dissolved in water, are not so when dissolved in alcohol.
6. The tendency of the precipitated metals to assume the granular or crystalline state:—lead, silver, and tin are precipitated from their solutions more readily than gold or platina.
7. The position of the reducing metal, relative to the solution:—when the metal is in plates or wires offering a large surface, the precipitation is rapid; but when it is placed at the surface of the liquor, touching it only by a point, it requires a long time. Two equal quantities of solution of silver were taken: a plate of copper was plunged into one, and a bar of copper made merely to touch the surface of the other; the first was entirely freed from silver in one hour, the latter not in three months.

When a zinc rod is put into a solution of acetate, or nitrate of lead, the first large crystals of lead after a time fall off, and are replaced by smaller, which in turn fall, and are succeeded by others, and this alternate production continues a long time. The cause of this effect appears to be in the power which most metallic solutions have, when saturated with base, of dissolving small quantities of other metals: thus the saturated nitrate, muriate, and acetate of zinc slowly dissolves a notable quantity of lead; the muriates of zinc and tin and the acetate of lead dissolve a little copper, and the nitrate of copper dissolves finely-divided silver. When, therefore, a piece of zinc is placed at the surface of a solution of lead, the latter metal falling to the bottom of the fluid is in part dissolved by the salt of zinc formed; and the solution of lead thus formed spreading through the liquid, thus

places the zinc in constant contact with it, and hence the successive precipitation of the lead which is found to take place.

If finely-divided silver is put at the bottom of a narrow tube, and about two inches in depth of a solution of copper saturated with oxide be poured upon it, then a copper-wire plunged to the depth of a couple of lines will soon become covered with silver, and in three or four weeks fine crystals of the metal will appear, which will be larger as the tube is narrower. It appears, that in all these circumstances double subsalts are formed; crystals of subnitrate of copper almost always appears mixed with those of silver.—*Ann. des Mines*, xii. 197.

14. *On the Solution of Gases in Water.*—Mr. Graham, in a review and consideration of the phenomena accompanying the solution of gases in water, has brought forward several ingenious arguments, to shew, that when gases appear to be absorbed by liquids, they are simply reduced into that liquid and comparatively inelastic form which otherwise (by cold or pressure) they might be compelled to assume. That their detention in the absorbing liquid is owing to that mutual affinity between liquids which is so common. An affinity which occasions the miscibility of liquids affects the bulk or density of the mixture, and frequently impairs the volatility of the more easily-vaporized liquid in the mixture. In this way the phenomena of the absorption of gases are brought into the same class as those of the miscibility of liquids.—*Ann. Phil. N. S.* xii. 69.

15. *On a new form of Carbon, supposed to be the pure Metallic Basis.*—Dr. Colquhoun has lately had occasion to observe the occurrence of a peculiar deposition of carbon, which takes place in Mr. Mackintosh's process of forming steel by passing gaseous compounds of carbon over ignited iron. The gas adopted in practice is carburetted hydrogen; the bars of iron are inclosed in an air-tight earthen-vessel, decomposition takes place at high temperature, carbon is deposited upon the metal, rapidly absorbed, and steel made, with great advantage, both as to economy and quality. When the gas is passed in such quantity as to deposit carbon in quantities surpassing that which can or ought to be absorbed by the iron, then accumulations take place, which are arranged in singular varieties of form, texture, and colour. The greater part of these have been met with in other situations, but one appears to be peculiar to the present apparatus. On opening, one day, that part of the apparatus where the carbon formation was to be discovered, there appeared, lying irregularly in various parts of it, a quantity of long capillary threads of carbon, lustrous and slender; each portion collected into a small bunch, as it were, of parallel lines, and in form extremely similar to a tress of fine

hair. A single lock of this mineral hair seemed to contain thousands of these thin filaments. There is nothing to which it can be better compared than to a bunch of fine glass-spun hair, or a tuft of one of the most perfect varieties of asbestos. As obtained at different times, the hairs are in length from an inch or less to eight inches. In thickness, some may equal a horse-hair, others the finest filaments of a spider's web. The colour has been always black, and the lustre bright and metallic. When the ends are bent together, the thread proves brittle, and snaps short across; when the point is pressed against the finger, the thread almost penetrates the skin before giving way; "and in so far as the eye of even the metallurgist could guide him, their whole appearance would indicate that they had been in a state of fusion at the moment of their formation." Sometimes the hairs were short, ragged, and uneven; sometimes quite destitute of lustre; and sometimes they existed so immingled in a loose mossy or spongy mass of carbon, as to be scarcely distinguishable. Dr. Colquhoun says he was never able to trace the formation of this uncommon product to any probable cause, nothing peculiar or uncommon about the working of the apparatus, or the state of the materials, being observed.

The experiments relative to the purity of this form of carbon seem to be very satisfactory. When heated in close vessels, they underwent no change; heated to redness in the flame of a candle, they remained unaltered; heated intensely in the interior of the blow-pipe flame, they burnt with occasional coruscations, and were entirely dissipated. When mixed with chlorate of potash or nitre, and heated, the mixture deflagrated. Rubbed up with peroxide of copper, and heated in a green glass tube, no trace of water could be observed, and consequently the absence of hydrogen inferred; and from other experiments, no foreign ingredient of a more fixed nature, whether earthy or metallic, seemed to make any part of it. Hence the result of all these experiments is, that the substance "is the *genuine basis of the gas*, or, in other words, *pure, solid*, and, to all appearance, *true metallic carbon*." Dr. Colquhoun observes, however, that it will only add one to the long list of forms, under which this substance is known to exist.

The substance was always remarked either half embedded among a mass of carbonaceous matter, or deposited upon the surface of such a mass, from which it would seem that a large excess of carburetted hydrogen had at those times passed through the apparatus.

The very numerous varieties of carbon aggregations found under different circumstances in coal-gas retorts, from the most hard and metallic to the soft pulverulent varieties, are now well known to chemists, their attention having been called to them by Mr.

Conybeare*. Dr. Colquhoun states, that he has more than once had occasion to remark the production of the filamentous variety which has just been described, as distinct from the others, in coke ovens. When the manufacture is complete, it is by no means uncommon to find the sides of the fissures in the mass of coke, encrusted by the mamellated varieties, and on the surface of the latter a small collection of delicate filamentous carbon; but very inferior to that found in the steel chest, its fibres being short, curled, and confusedly interlaced among each other, and generally without lustre.

Dr. Colquhoun remarks at some length upon the *apparent* fusion which the carbon seems to have undergone, and upon the difficulty of conceiving how at such temperatures that could really have happened.

Mr. Brayley, in a subsequent paper of the *Annals*, suggests, that probably the carbon, upon being evolved from its combination with the hydrogen in the gaseous, passes through the liquid form before it attains solidity. He gives his reasons, from the experiments of M. C. de la Tour and others, for believing that the liquid and the gaseous states are in fact the same; and considers that there is no conversion of a solid into vapour or gas, or of the latter into the solid form again, without the intervention of the fluid state.—*Ann. Phil. N. S.* xii. 192.

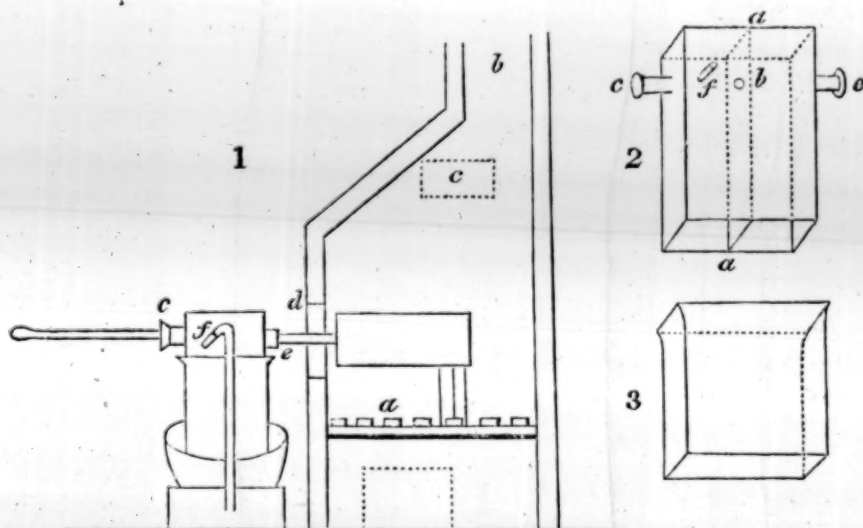
16. *Iodine in Mineral Waters.*—Mr. Turner has found iodine in the water of the Bonnington Spring, near Leith. It is easily detected in the following manner: evaporate a pint of the water to dryness, take up the soluble parts in a drachm or two of a diluted solution of starch quite cold, and add a few drops of concentrated sulphuric acid; the characteristic blue colour will then make its appearance. Mr. Turner prefers sulphuric acid to nitric acid or chlorine, for decomposing the hydriodic acid; for it effects that object with certainty, and does not decompose the iodide of starch, or prevent its formation, as the two last are apt to do.—*Edin. New Phil. Journ.* i. 159.

17. *Advantageous Method of preparing Potassium*, by M. Wohler. —This process is a modification of that devised by M. Brunner †, and is the one by which such large quantities of potassium have been prepared abroad. One of those wrought-iron bottles in which mercury is carried, is to be heated red hot, to expel any portions of mercury that may adhere to it, and is then to be one-third filled with carbonized cream of tartar. In carbonizing the cream of tartar (and crude tartar may be used), five or six pounds are to be heated in a covered crucible until no more gas is disengaged; this is to be pulverized whilst hot, mixed with a few ounces of

* *Ann. Phil. N. S.* v. 50.

† See Vol. XV. p. 379, of this Journal.

charcoal, and introduced into the iron bottle before it is cold, lest it should attract moisture. A piece of gun-barrel, about a foot in length, is then to be screwed into the bottle, and the latter is to be placed horizontally in a wind furnace, such as is repre-



sented in the figure: *a* is the grate; *b*, the flue, which should have a height of twelve or fifteen feet; *c*, an opening for the introduction of fuel; *d*, an opening by which the apparatus is introduced, and which is to be afterwards closed by bricks. The retort is to be placed horizontally on a brick at the height of six inches, and it is essential that the anterior part of the retort should almost touch the front of the furnace, so that the gun-barrel may be short, and may not be too much heated.

The retort is to be heated; when red hot, water will be disengaged, and after that oxide of carbon; when it has acquired a white heat, a dense fume will issue forth, alternately white, gray, brown, green, and nearly black; this fume is very combustible, and burns with a brilliant flame. Almost immediately afterwards the retort will be seen full of a greenish gas, and it is then time to put the receiver, in which the potassium is to be condensed, in its place.

This receiver is the invention of M. Berzelius. It may be made of copper or sheet-iron. It is a foot in height, six inches wide, and one and a half inches thick. It is composed of two pieces (see the figures 2, 3), the first of such size as to pass into the second with some degree of friction; it is divided into two equal parts by a diaphragm, *a a*, pierced by a small hole, *b*, on the same line with the tubulars, *c* and *e*; on one of its larger faces is a third tubular, by which the gases are allowed to escape. Naphtha is to be put into the receiver until about half full, and the latter is then to be placed in a vessel containing ice or water, constantly retained at a low temperature; finally, the tubular *e*, is to be adapted to the end of the gun-barrel, and the joint made tight by

a little lute if necessary. The gas, generated in the retort, enters the receiver by the tubular *c*, and as it is obliged to pass by the very small aperture at *b*, it deposits the greater part of its potassium before it passes out by the tube, *f*; it also deposits a certain portion on the other side of the diaphragm; and finally, to remove the last portion which it may contain, a large glass tube is connected with the aperture at *f*, and its extremity plunged into naphtha.

The operation is known to proceed well by the regularity of the current of gas disengaged by the tube *f*; when this disengagement diminishes, it is in consequence of obstruction of the gun-barrel by a black substance accumulating in it, which appears to be a compound of potassium, carbon, and potash, resulting from the re-action of the potassium on the oxide of carbon. The tubular *c*, is then to be opened, it being otherwise closed by a cork; and by passing a ramrod, flattened at one end, through the apertures *c*, *b*, and *e*, the tube is to be cleared out as much as possible. This must be done cautiously, or otherwise a sudden expansion of gases may take place at the opening *e*, *f*, and cause serious accidents. M. Gmelin places the diaphragm of his receiver in the direction of its length, and is thus enabled to introduce the blade of a sword, by which he can clear the tube much more readily than with a rod.

The tube may easily be cleared two or three times, but after a certain period it becomes so plugged up, that the operation must be suspended. In such case, withdraw the bars of the grate, and let the fuel fall; remove the retort, unscrew the tube, fill it with naphtha, and detach the substance which it contains; re-attach it, replace the retort in the fire, and proceed to distil the remainder of its contents. In this way the operation may be repeated three or four times, and on from four to six pounds of tartar, each time about an ounce of potassium will be obtained.

On distilling the black matter detached from the iron tube, and that which sometimes passes into the receiver, the half of its weight of potassium may be obtained.

The potassium, thus prepared, contains carbon in combination. To obtain it pure, it must be distilled, by small portions at a time, in a glass retort, heated to redness in a sand-bath; the neck being very much inclined and plunged into a vessel full of naphtha.—*Annales des Mines*, xii. 175.

18. *On a new Acid and Salts obtained during the preparation of Potassium.* By M. Gmelin.—When potassium is prepared according to Brunner's method *, the iron tube is rapidly filled with a grey substance, which can only be detached by a chisel, and a gas is evolved which deposits a greyish-brown substance on cold

* See Vol. XV., p. 379, of this Journal.

bodies, and burns with a reddish, bright flame. The same substances are obtained when a mixture of charcoal and carbonate of potash is used; that which fills the tube is of the same nature as that deposited from the gas, except that it is mixed with potassium, carbonate of potash, and charcoal. The brown substance, with a small quantity of water, separates into two parts; the one soluble, and yielding a brownish-yellow liquid; the other of a fine red colour, soluble only in a large proportion of water: it usually contains a little carbon. The solution rapidly evaporated yields only a brown carbonate of potash; but spontaneously concentrated, it furnishes an acicular orange-coloured salt, and towards the end of the operation, bi-carbonate of potash. The acicular salt is easily purified by repeated crystallization, and has been called by M. Gmelin *croconate of potash*, because it contains a yellow acid, which yields many combinations of the same colour.

Croconate of potash is neutral, inodorous, having a weak taste, like that of nitre; its primitive form is a rhomboid of 106° and 74° .

When heated, the salt first loses water of crystallization, then decrepitates, and is decomposed; a slight odour of naphtha is evolved, and carbonic oxide and acid, with, perhaps, a little hydrogen, produced; the residue, which retains the form of the salt, is a carbonate of potash and charcoal.

The salt is very soluble in water, more so at high than low temperatures; it crystallizes on cooling. It is insoluble in strong alcohol, and yields its water of crystallization to it. Dry gaseous chlorine has no action on it, even at temperatures which evolve the water of crystallization; but the gas immediately destroys the colour of the salt when in solution, and appears to transform it into another salt, not as yet examined. Iodine has a similar action upon it.

Cold, concentrated sulphuric acid immediately removes the water of crystallization; by a gentle heat solution takes place, and on cooling, pale-yellow transparent crystals are deposited. If the heat be strong, the salt is decomposed, with swelling; sulphurous acid is disengaged, and by a red heat all the charcoal is burnt, and sulphate of potash left. Nitric acid dissolves the salt, with effervescence; a colourless solution is produced, which yields by evaporation a confusedly-crystallized pale-yellow mass, soluble in water, and giving with potash, baryta, and lime, the same salts apparently as those produced by chlorine; no oxalic acid is formed.

The *croconic acid* is easily obtained by treating this salt with absolute alcohol, to which a little sulphuric acid has been added; a sulphate of potash is formed, and the croconic acid is dissolved. The acid cannot be well obtained from the croconate of lead by sulphuric acid, or sulphuretted hydrogen, or from the croconate of

silver by muriatic acid: in the first case, sulphuric acid is always mixed with it; in the second, sulphur; and in the third, potash.

Croconic acid crystallizes in grains or needles; it is transparent, of a fine yellow colour, inodorous, of a rough acid taste, and strongly reddens litmus. It may be heated to 100° (212° F.) without alteration; but, at a higher temperature, is altered, blackens and burns, leaving no residuum. It is very soluble in water and alcohol, forming lemon-yellow solutions.

On analysing the salt of potash, it was decomposed by heat, by oxide of copper, and the potash estimated by sulphuric acid. Though M. Gmelin found no hydrogen, he thinks that everything tends to prove that a small quantity is present, and that the acid of the salt is an *hydracide* analogous to the prussic acid, and that it ought to be called the hydro-croconic acid, it being probable that its basis is combined with potassium in the matter deposited by the gas, and that the water changes the *croconide* into a *hydro-croconate*. The most probable composition of the salt is—

		Prop.	Calculated results.	Experimental.
Hydro-croconic acid.	Potash . .	1=48	.3721	.3681
	Carbon . .	5=30	.2323	.2380
	Oxygen . .	4=32	.2481	.2554
	Hydrogen . .	1=1	.0077	
	Water . .	2=18	.1398	.1518
			1.0000	1.0133

The grey matter which accompanies the potassium is either formed in the iron vessel by the action of the carbon on the carbonic acid of the alkaline carbonate, or in the tube, and in all parts where the condensation takes place. The last supposition is the most probable, as giving a reason for the presence of the carbonate of potash. Nevertheless, M. Gmelin says, he was not able to form carbonate of potash by making potassium act upon oxide of carbon: perhaps, he observes, this salt is only formed at determinate temperatures.

It is impossible to obtain the red matter before spoken of pure; it always contains a certain quantity of croconate of potash. When carefully dried, it is pulverulent, and of a fine cochineal-red colour. When sufficiently heated, it burns with flame and smoke, evolving an empyreumatic odour, and leaving a difficultly-combustible charcoal. When this is burnt away, pure carbonate of potash remains. The solution of the substance is immediately decoloured by nitric acid. It precipitates baryta-water brown, lime-water pale reddish, and solutions of tin, lead, and mercury yield reddish precipitates with it; whilst silver gives a reddish-black one, &c.

As this red matter is changed into croconate of potash by feeble

oxygenating agencies, it appears to be composed of potash, combined with an acid, which differs from croconic acid only in containing less oxygen.—*Ann. des Min.* xii. 164.

19. *New Compounds of Chromium with Fluorine and Chlorine.*—M. Unverdorben found that by distilling fluor-spar, chromate of lead, and sulphuric acid in a leaden retort, a gas was evolved, which was readily absorbed by water, and proved to be a mixture of chromic and fluoric acids. This gas gave a thick yellow or red smoke, and on coming in contact with air deposited small red crystals of chromic acid. Berzelius collected this gas in glass vessels coated with resin, because glass is quickly destroyed by it, and even the resin is gradually attacked throughout to the very glass, chromic acid being deposited. Berzelius found that ammoniacal gas introduced into this gas, burns with explosion; crystals of chromic acid introduced into ammoniacal gas, are decomposed with a flash of light, the metal being left in the state of protoxide. Water, by absorbing this gas, acquires an orange tint; and this, by evaporation, affords chromic acid, the fluoric acid being volatilized: this is the only method known of obtaining pure chromic acid. The gas, received in a deep and moistened platinum vessel, descends, the water becomes saturated with it, and then is entirely absorbed by the fluoric acid, which is at length dissipated, and the vessel is filled with a red snow, consisting of chromic acid. These crystals, when heated to redness in a platinum dish, fuse, explode with a flash, and resolve themselves into protoxide and oxygen; but the crystals which the water had absorbed do not present this phenomenon. If chromate of lead be distilled with common salt and anhydrous sulphuric acid, we obtain a reddish gas, consisting of chrome combined with chlorine. Water converts it into chromic and muriatic acids.—*Brewster's Journal*, iv. 129.

20. *On some new Compounds, Fluorides, Chlorides, &c.* by M. Dumas.—This highly-interesting notice is contained in a letter from M. Dumas to M. Arago, *Annales de Chimie*, xxxi. 433.

I have latterly made some observations which my occupations have prevented me from developing sufficiently, and as they are principally destined to illustrate points of theory which require very delicate experiments, I am afraid it will be some time before I can publish them. The object of my labours is to arrive at a determination of the atomic weight of each body by the density of its vapour; this has been already done for some substances. I have published researches on phosphorus, which establish the density of its vapour, and have made an analogous series of experiments on arsenic. I shall soon send you results of the same kind, deduced from the density and composition of the following

bodies,—fluoboric acid, fluosilicic acid, bi-chloride of tin, chloride of arsenic, chloride of antimony, proto-chloride of phosphorus, telluretted hydrogen, seleniuretted hydrogen. I have submitted all these bodies to a new examination.

I have wished to include in my experiments a large number of metals, but was prevented by the difficulty of finding known volatile metallic combinations. I examined whether the great tendency of fluorine to form gaseous combinations would not give it the property of volatilizing the metals. To produce these new compounds, I treated the fluoride of mercury, or of lead, by bodies more positive than these metals, or else I put nascent fluoric acid in contact with the oxides of the bodies which I wished to combine with the fluoric radical. In my first efforts I obtained gases which contained much fluorine and arsenic, fluorine and tin, fluorine and antimony, &c.; but I soon perceived that these gases were mixtures of silicated fluoric acid, and a metallic fluoride in the state of vapour: finally, I procured the *fluoride of arsenic* in the liquid form; it exactly resembles the fuming liquor of Libavius, evolves fumes in the air, is very volatile, heavier than water, becoming fluoric and arsenious acids, when acted upon by water. It scarcely acts on glass, and may be preserved for a long time in glass vessels; but on the skin exerts an action as powerful as that of fluoric acid itself. If a small drop be put upon the skin, although it is volatilized almost immediately, like ether producing cold, the part touched is deeply burnt, and becomes a wound, suppurating slowly, and difficult to crystallize. Its vapour produces similar effects, and occasions those particular pains under the nails experienced by Sir H. Davy, after being exposed to the vapours of concentrated fluoric acid. Having been wounded in collecting a quantity of this product, I was obliged to attend to the cicatrization of the burn before proceeding to examine the properties of the substance; but I am able to say, that the density of its vapour is at least four times that of air. The discovery of this body will give me the means of ascertaining the density of fluorine, and afterwards those of boron and silicium, knowing already the density of the vapour of arsenic.

The *fluoride of antimony* is solid at ordinary temperatures, white as snow, more volatile than sulphuric acid, but less so than water. Its composition corresponds to that of the protoxide and butter of antimony.

The *fluoride of phosphorus* is a colourless, fuming liquid, easily obtained in abundance by operating on fluoride of lead with phosphorus. Its composition corresponds to the proto-chloride of phosphorus. In the same way may be obtained the fluoride of sulphur. These are compounds of the same kind as those obtained by M. Unverdorben, by treating the chromate of lead with sulphuric acid and fluuate of lime, or common chalk. He believed,

as I at first thought, that these compounds were permanent gases, but that is not the case.

The *chloride of chrome*, corresponding to chromic acid, and as obtained by his process, is a liquid of a fine blood-red colour, heavier than water, very volatile, fuming in the air, and then of a colour analogous to that of nitrous acid, when in vapour. It rapidly attacks mercury, acts on sulphur with much energy, and a hissing noise; it detonates with phosphorus, and the action is accompanied with the evolution of heat and light, even when operating with a drop of the fluid and a piece of the solid the size of a pin's head only. It has no action on charcoal, and dissolves iodine. It absorbs chlorine also, and becomes pasty and almost solid. This new matter is brown: when dissolved in water, it loses its excess of chlorine, and the action takes place with decrepitation. In the air it evolves thick red fumes, having an odour something resembling that of iodine. In the experiment of M. Unverdorben, this substance, chlorine, and muriatic acid are disengaged at the same time; by passing the products through a cold tube, the chloride of chromium is entirely condensed.

These trials are as yet imperfect, but as the existence of metallic fluorides, similar in their composition and properties to the analogous chlorides, puts the reality of M. Ampère's hypothesis on the nature of the fluoric radicle beyond a doubt, I hope that chemists will pardon this early communication.

Permit me to add a word on the new combination of boron. A mixture of borax and charcoal being put into contact with dry chlorine at a red heat, yields abundance of the chloride of boron. This is a very fuming gas, and very soluble in water. I have taken advantage of it in the analysis of boracic acid; for when it decomposes water, it gives muriatic and boracic acids. But its most important property is that of forming a solid hydrate, susceptible of being reduced by hydrogen and the heat of a spirit-lamp; it becomes muriatic acid and boron, and in this way the latter substance may be procured in large quantities.—Paris, May 6, 1826.—*Ann. de Chim.* xxxi. 433.

21. *Peculiar States of Sulphate of Baryta and of Water*, observed by Dr. Brewster.—In a note to a paper on the properties of the two new fluids found in minerals, Dr. Brewster states a very extraordinary fact relative to sulphate of baryta. He says, "Mr. William Nicol, lecturer on natural philosophy, has shewn me a very remarkable specimen of *sulphate of baryta*, with fluid cavities of the same general character with those which I have described in my former paper, but much larger than any which I had seen. Upon grinding down on a dry stone one of the faces of this specimen, the largest cavity burst, and discharged its fluid contents through the fissure upon the ground surface of the

specimen. The fluid lay in drops of different sizes along the line of the fissure, and in this condition Mr. Nicol put it into his cabinet. Upon looking at the specimen about *twenty-four* hours afterwards, *each drop of fluid had become a crystal of sulphate of baryta*. These crystals had the primitive form of the mineral."

"This very curious fact is analogous to the uncrystallized water in the ice-cavities mentioned above, the crystallization in both cases being prevented by pressure*. When that pressure was removed, a portion of the water and the fluid sulphate of baryta were crystallized immediately. Mr. Nicol distinctly remarked that the crystals occupied nearly as much space as the drops of the fluid; so that the crystals of sulphate of baryta were not deposited from an aqueous solution, but bore the same relation to the fluid from which they were formed as ice does to water."

The phenomena of ice-cavities referred to are thus mentioned: "I have sometimes observed frozen drops of dew containing a portion of water which *remained unfrozen even at low temperatures*; and I have recently had occasion to examine some crystallizations of ice which presented the same fact, under more curious circumstances. A very sharp frost occurred in Roxburghshire on the morning of October 9, 1825, the gravel-walks in the garden were raised up about an inch above their natural level, by the sudden congelation of the water in the earth mixed with gravel. All the elevated portions consisted of vertical prismatic crystals of ice, of six-sided prisms, with summits which seemed to be triedral. The leaves of plants were covered with granular crystals, which were in general six-sided tables. Upon examining with a microscope, the prismatic crystals aggregated in parallel directions, they presented some curious phenomena. They had numerous cavities of the most minute kind arranged in rows parallel to the axes of the crystals, and at such equal distances as to resemble a series of mathematically-equidistant points; some of the cavities were very long and flat, and sometimes they were amorphous; but in general they contained water and air." Dr. Brewster then describes, that when from fusion one side of the crystal became weak, a portion of the included air passed through the solid ice, and its place was immediately filled up by ice, and the dimensions of the cavity thus reduced.—*Brewster's Phil. Journal*, v. 135.

22. *On the Mode of Action of Puzzolanas and Hydraulic Mortars*, by M. Vicat.—The following important experiments on the theory of puzzolanas and hydraulic mortars, have been made by M. Vicat, in illustration of his views of this subject. Some anomalous appearances occurred when cements were prepared with pure silica and pure alumine in different states, and mixed with lime, the results not agreeing with those which were obtained, when

* Mr. Perkins says crystallization is occasioned by pressure.

instead of these bodies a clay was itself treated in a similar way. For these and other reasons, different views had been taken of the kind of action by which such mortars or cements were enabled gradually to aggregate and become hard. MM. John and Berthier attribute the phenomena exclusively to solidification, but M. Vicat considers them as principally dependent upon the mutual action of the solid matter present. Upon trials with slightly-calcined clay which was afterwards pulverized, it appeared that any absorption of water which took place was almost entirely instantaneous, but very little effect being afterwards produced; and upon making the experiments with a good and a bad puzzolana, this slight effect differed only by a tenth; from which M. Vicat concludes that the solidification of hydraulic cements containing puzzolana, cannot be attributed exclusively to an absorbing power joined to the hardness of the particles of matter.

These considerations suggested the impropriety of assimilating that which passes in an intimate mixture of silica, alumina, and oxide of iron, when slightly calcined, to that which takes place when the same substances are calcined separately; and that perhaps it was erroneous to state as a general result, that clay, in its natural state, entered more readily into chemical combinations, than when baked to a certain degree. These conjectures were changed into proofs by the following experiment:—A certain quantity of pure lime-water was put into a bottle, and then portions of the best artificial puzzolana, which M. Vicat had used for twelve years past, introduced. Being well agitated and left to settle, a few drops of the fluid were withdrawn, filtered, and tested by sub-carbonate of potash, and the presence of lime ascertained. More puzzolana was added, and the liquid again tested; this time the precipitate was much less, and it ceased altogether, when the proportion of lime-water to the puzzolana was as 7 to 1. The same experiment was made on the same clay as that from which the puzzolana had been made, in its unbaked state; 1.84 parts of lime-water had not been freed from lime by 1.21 parts of the clay (equivalent to 100 of the dry clay,) and then the experiment was stopped, because the clay was in such quantity as no longer to allow of the separation of the water.

This fact, according to M. Vicat, resolves all the difficulties, by shewing that calcined clay has a stronger affinity for lime than the clay in its natural state. It is to be remarked that the above experiments only occupied an hour and a half, that the puzzolana was in visible grains, and that the lime which entered into combination was, and could only be, a very small fraction of that which enters into combination in hydraulic mortars of two or three years old.

M. Vicat then examined whether the energy of a puzzolana was proportional to the quantity of lime-water which it was ca-

pable of neutralizing in a certain time. The experiment was made therefore with a very bad puzzolana, prepared by heating an argillaceous schist to redness, and it was found that 100 parts of this substance neutralized only 66 parts of lime-water in the same time that 100 parts of good puzzolana neutralized 700 parts; the ratio, therefore, thus afforded, is 700 to 66. By referring back to early experiments on the hydraulic mortars made with these puzzolanas and ordinary lime, it was found that, when examined after the lapse of one year, their resistances were as 640 to 97. The difference in these results will not appear extraordinary to those who are acquainted with the various difficulties accompanying these kinds of experiments; and M. Vicat concludes that lime-water, used as described, may become a simple and expeditious method of measuring the energy of various puzzolanas employed in constructions. . Whatever may, however, be the result of such trials, it is demonstrated, 1. That clays which by a slight calcination become good puzzolanas, are also puzzolanas, though in a less degree, in their natural state. 2. That the solidification of hydraulic cements formed of ordinary mortar and calcined clays, is the result of a true combination, in which the lime is neutralized by the silica and alumina.

As the effect is the result of an internal action, no extraneous matter being present, this action continuing for many years after the first solidification, it follows that a molecular movement takes place in the solid mass, and thus another is added to the many instances of this kind already known.—*Annales de Chimie*, xxxii. 197*.

23. *On Starch, and some of its Commercial Varieties, as Sago, Tapioca, &c.*, by M. Caventou.—In a memoir on the varieties of starch existing in commerce, M. J. B. Caventou has examined them chemically, with great attention. He observes that the insolubility of starch in cold water is well known, and that generally the paste formed by heating starch and water together, is considered as a hydrate of starch. He thinks, however, that a greater change than this has taken place, and that as the starch has lost its distinguishing character, that of insolubility in cold water, it can no longer be considered the substance it was before, and he observes also upon the impossibility of obtaining the starch back again in its former state.

Starch-paste (*empois*), he considers as of two kinds; that with a *minimum* of starch, which is quite transparent, or very slightly opalescent, and that with a *maximum* of starch, which is nearly or quite opaque. The first kind is almost entirely soluble in water, leaving only a slight residue of pure starch. The solution has

* See Vol. X. p. 407, and Vol. XIX. p. 329.

the properties usually attributed to a solution of starch, and amongst others, that of leaving, upon evaporation, a transparent yellow substance resolvable in cold water, for which reason M. Caventou contends it cannot be starch.

With a view of ascertaining whether the change produced was due to the water or to the temperature, or to both, and in what proportion, starch was subjected for a long time to temperatures from 140° to 158° , F., which with water is sufficient, but without it produced no effect, not rendering it, when cold, soluble in cold water; but when raised to 212° and above, near to the heat at which it decomposes, it acquired a feeble red tint, gave out an odour of baked bread, and when cold, dissolved in cold water, forming a solution exactly like that obtained with hot water; so that, it appears, water by its dissolving power will compensate for many degrees of heat.

At a higher temperature, the starch is quite altered, dissolves with great facility in water, but gives a purple tint with iodine instead of a blue one.

The second kind of starch-paste differs from the former merely by containing much unaltered starch. Starch-paste therefore is a ternary mixture of pure starch, modified starch and water, and all are necessary to make good paste. If the paste be boiled for a long time, the water being renewed as it evaporates, and the fluid at last evaporated, a hard, horny, transparent substance is obtained, as Vogel has shown, resolvable in water, and containing no pure starch.

The starch thus altered, M. Caventou considers the same as that substance called by Saussure *amidine*. He thinks that Saussure was mistaken in his views of the manner in which his amidine had been formed, it having been produced in fact in the usual way, by heating pure starch with water, and not by the effect of time on starch-paste.

When a solution of amidine is boiled for a long time, it becomes a far more soluble substance, and is now coloured purple by iodine, not blue. The same change is effected in starch by strong torrefaction, or by ebullition, with a mixture of 1 part sulphuric acid, and 12 parts of water. By still longer ebullition, iodine loses the power of producing colour altogether.

Salop mixed with cold water, and filtered, gave a gummy, transparent liquid; precipitating, by nitrate of silver, oxalate of ammonia, and subacetate of lead; evaporated, it left a viscid matter like gum, unchanged by iodine, and precipitated by alcohol. When burnt, it left common salt and phosphate of lime. The gelatinous, trembling, insoluble mass left on the filter was treated with boiling water, and being filtered, a solution was obtained precipitable of a fine blue, by iodine; but a very small portion of starch was in this way abstracted, and a large quantity of a gelatinous,

transparent, bulky substance remained; insoluble in water, soluble in muriatic acid, with nitric acid furnishing oxalic acid, and in fact having all the properties of *bassorine*. Thus salop is composed of a little gum, very little starch, and much *bassorine*. 100 parts yielded 4 parts of fixed substances, these being common salt, phosphate of lime, and a little sulphate of lime.

Sago, digested in cold water, and filtered, gave a clear solution, precipitable of a fine blue colour by iodine, the blue substance having the properties of the iodide of starch. By further experiments, it appeared that sago is an uniform substance, soluble in cold water, more soluble in hot water, precipitated blue by iodine, but differing from ordinary starch by its solubility in water at common temperatures.

Tapioca presented the same phenomena as sago, being entirely soluble in cold water. It appeared to be identical with that substance, and neither of them seem to contain any pure starch according to Mr. Caventou's notions of that substance.

Arrow-root is nearly pure starch, and in all respects agreed with the starch of the potato.

From a consideration of the process by which sago and tapioca are prepared, M. Caventou concludes, that the differences between them and potato-starch depend upon the manner in which they are extracted and prepared; that the change is probably effected by the heat which is applied, in one part of the process, to granulate and dry the substances; that the change is easily effected with potato-starch, and that thus one of the most striking distinctions of amylaceous matter is destroyed.—*Ann. de Chimie*. xxxi. 337.

24. *On the Distillation of Fatty Bodies*.—Those chemists who take an interest in the changes effected in this important class of bodies by heat, and have read the results obtained by M. Dupuy*, may find a second memoir on the same subject, and by the same person, in the *Annales de Chimie*, xxxii. 53. The latter experiments were made to clear up certain doubts which had existed relative to the results previously obtained, and are confirmatory of the facts first described. The conclusions are, 1. That if fat be distilled by evaporation (without ebullition,) the product is more solid than that obtained by boiling distillations; and 2, That the length of time occupied in a boiling distillation has very great influence on the solidity of the product obtained.

25. *Purification of Pyroligneous Acid by Animal Charcoal*.—Pyroligneous acid, purified by the usual methods, contains a certain quantity of empyreumatic oil; this gives it an odour that is obliged

* See Vol. XX. p. 391, of this Journal.

to be covered by the addition of a small quantity of essence of roses. I have found from experiment, that the minutest trace of this oil may be removed by means of the animal charcoal which is produced in the manufactories of Prussian blue. It is only necessary to digest the acid with the charcoal, and filter it.—(Berzelius. *Ann. de Chimie*, xii. 338.

26. *On Proving the Colours of Dyed Stuffs.*—Proofs are either natural or artificial. The natural proof consists in exposing the dyed stuff to the air, sun, and rain. If the colour is not changed by this exposure in twelve or fourteen days, it may be considered as fixed. This proof, however, is not adapted for every colour, because some colours will stand it, and yet fade in consequence of the application of certain acids; and there are other colours that cannot resist the natural proof, and yet remain unchanged by acids. Colours, therefore, may be arranged in three classes, and to each of them a different kind of artificial proof is allotted. The first class is tried with alum, the second with soap, and the third with tartar.

For the proof with alum, half an ounce of that salt is dissolved in a pint of water, in an earthen pipkin, and into this put half a quarter of an ounce of the dyed thread, or stuff, that is to be tried, and the whole being boiled for about five minutes, it is then washed with clean water. In this manner are tried crimson, scarlet, flesh-colour, violet, ponceau, peach-blossom colour, different shades of blue, and other colours bordering upon these. For the proof with soap, a quarter of an ounce of soap is to be boiled in a pint of water, and half a quarter of an ounce of the dyed thread or stuff that is to be tried, is put into the liquor, and suffered to boil for five minutes. With this proof all sorts of yellow, green, madder-red, cinnamon, and similar colours are tried. For the proof with tartar, that substance must be pounded very small, that it may be more easily dissolved; an ounce is then boiled in a pint of water, and a quarter of an ounce of dyed thread or stuff is boiled in the solution for five minutes. This proof is used for all colours bordering upon the fallow or hair brown.—Franklin's *Journal*, ii. 55.

27. *Instrument for ascertaining the Presence of Animal Matter in the Atmosphere.*—Dr. Granville is stated to have invented an instrument which, by means of a preparation of chlorine, enables him to ascertain not only whether animal matter, in a state of decomposition, be floating in the air, but also the quantity of such animal matter; a knowledge which cannot be obtained by the usual apparatus for analyzing atmospheric air, and the importance of which, to the medical profession, must be obvious. He proposes calling the instrument the *Septometer*.—*Med. Jour.* lvi. 198.

28. *A method of Fixing Chalk and Pencil Drawings.*—The beauty and freshness of a chalk or pencil drawing depends so much upon the perfectly-undisturbed state of the artist's touches, that they frequently disappear by such slight friction and rubbing as would, by an inexperienced person, be supposed to produce no change in the lines. To preserve them from this deterioration, and from the partial obliteration dependent upon longer rubbing in a portfolio, artists are anxious to fix their drawings, and by giving a permanency to the lines, not otherwise possessed by them, enable them to resist these changes. For this purpose washes, consisting of weak size, dilute jelly, or milk, have been used; but these are all far surpassed by one, for which the artists are indebted to Mr. Hatchett, of Belle-Vue House, Chelsea. It is a weak solution of gum-mastic in alcohol, the proportions being ten grains of the mastic, and one ounce of rectified spirit of wine. The gum is to be pulverized, put into the spirit, and left for four or five days, at the end of which time a clear solution will be obtained. The chalk or pencil drawing is to be held in an inclined position over a plate, and this solution poured over its surface. It is then allowed to drain, and is to be dried spontaneously in the air. It sometimes happens that the lower edge has a slight tint given to it by the accumulation of matter there, during the draining and drying. This is generally of no consequence; should it be desirable to avoid it, it is easily removed whilst the drawing is wet, by passing a hair-pencil dipped in alcohol along it, which removes the excess of matter.

The alteration upon the surface of the paper by this process is so slight, that no appearance of change can be perceived, no change of colour, no lustre, and no adhesion of dirt being occasioned; at the same time the drawing is so permanently fixed, that it may be rubbed with India-rubber without suffering any change or obliteration. The extent to which this is effected is well illustrated by fixing the one-half of a drawing or writing, and, when dry, passing India-rubber over it; the one-half will be removed, and the other remain unaltered.

29. *On the confinement of Dry Gases over Mercury.*—The results of an experiment made by myself, and quoted as such, having been deemed of sufficient interest to be doubted, I have been induced to repeat it, and though the original experiment was not published by me, I am inclined to put the latter and more careful one upon record, because of the strong illustration it affords of the difficulty of confining dry gases over mercury alone. Two volumes of hydrogen gas were mixed with one volume of oxygen gas, in a jar over the mercurial trough, and fused chloride of lime introduced, for the purpose of removing hygrometric water. Three glass-bottles, of about three ounces capacity each, were selected

for the accuracy with which their glass stoppers had been ground into them; they were well cleaned and dried, no grease being allowed upon the stopper. The mixture of gases was transferred into these bottles over the mercurial trough, until they were about four-fifths full, the rest of the space being occupied by the mercury. The stoppers were then replaced as tightly as could be, the bottles put into glasses in an inverted position, and mercury poured round the stoppers and necks, until it rose considerably above them, though not quite so high as the level of the mercury within. Thus arranged they were put into a cupboard, which happened to be dark, and were sealed up. This was done on June 28, 1825, and on September the 15th, 1826, after a lapse of fifteen months, they were examined. The seals were unbroken, and the bottles found exactly as they were left, the mercury still being higher on the inside than the outside. One of them was taken to the mercurial trough, and part of its gaseous contents transferred; upon examination it proved to be common air, no traces of the original mixture of oxygen and hydrogen remaining in the bottle. A second was examined in the same manner; it proved to contain an explosive mixture. A portion of the gas introduced into a tube, with a piece of spongy platina, caused dull ignition of the platina; no explosion took place, but a diminution to rather less than one-half. The residue supported combustion a little better than common air. It would appear, therefore, that nearly a half of the mixture of oxygen and hydrogen had escaped from it, and been replaced by common air. The third bottle, examined in a similar manner, yielded also an explosive mixture, and upon trial was found to contain nearly two-fifths of a mixture of oxygen and hydrogen, the rest being a very little better in oxygen than common air.

There is no good reason for supposing that this capability of escape between glass and mercury is confined to the mixture here experimented with; probably every other gas, having no action on the mercury or the glass, would have made its way out in the same manner. There is every reason for believing that a small quantity of grease round the stoppers would have made them perfectly tight. M. F.

III. NATURAL HISTORY, &c.

1. *Description of the Great Crater of Kirauea on the Island of Hawaii (Owhyhee).*—This description is from the Journal of a Tour around Hawaii, the largest of the Sandwich Islands, by a Deputation from the Mission on those Islands, published at Boston, 1825. The crater described is situated about twenty miles from the sea-shore, in the interior, and is thus described: "Im-

mediately before us yawned an immense gulf, in the form of a crescent, upwards of two miles in length, about a mile across, and apparently eight hundred feet deep. The bottom was filled with lava, and the south-west and northern parts of it were one vast flood of liquid fire, in a state of terrific ebullition, rolling to and fro its 'fiery surge' and flaming billows. Fifty-one craters, of varied form and size, rose like so many conical islands from the surface of the burning lake. Twenty-two constantly emitted columns of grey smoke, or pyramids of brilliant flame, and many of them at the same time vomited from their ignited mouths streams of florid lava, which rolled in blazing torrents down their black, indented sides, into the boiling mass below.

"The sides of the gulf before us were perpendicular for about four hundred feet, when there was a wide horizontal ledge of solid black lava, of irregular breadth, but extending completely round. Beneath this black ledge the sides sloped towards the centre, which was, as nearly as we could judge, three or four hundred feet lower. It was evident that the crater had been recently filled with liquid lava up to this black ledge, and had, by some subterranean canal, emptied itself into the sea, or inundated the low land on the shore. The grey, and, in some places, apparently calcined sides of the great crater before us, the fissures which intersected the surface of the plain on which we were standing, the long banks of sulphur on the opposite side, the numerous columns of vapour and smoke that rose at the north and south end of the plain, together with the ridge of steep rocks by which it was surrounded, rising probably in some places four hundred feet in perpendicular height, presented an immense volcanic panorama, the effect of which was greatly augmented by the constant roaring of the vast furnaces below.

"Between nine and ten the dark clouds and heavy fog that, since the setting of the sun, had hung over the volcano, gradually cleared away, and the fires of Kirauea, darting their fierce light athwart the midnight gloom, unfolded a sight terrible and sublime beyond all we had yet seen. The agitated mass of liquid lava, like a flood of melted metal, raged with tumultuous whirl. The lively flame that danced over its undulating surface, tinged with sulphureous blue, or glowing with mineral red, cast a broad glare of dazzling light on the indented sides of the insulated craters, whose bellowing mouths, amidst rising flames and eddying streams of fire, shot up at frequent intervals, with loud detonations, spherical masses of fusing lava, or bright, ignited stones. The dark bold outline of the perpendicular and jutting rocks around formed a striking contrast with the luminous lake below, whose vivid rays, thrown on the rugged promontories, and reflected by the overhanging clouds, combined to complete the awful grandeur of the imposing scene."

It is a striking feature of this volcano that it does not spring out of a mountain or hill, as is the case, we believe, in all other parts of the world, but is seated in a comparatively plain country, or rather at the base of the stupendous mountain Mouna Roa. It never overflows its margin, like other volcanoes, but the lava seeks a subterraneous passage, bursting out occasionally at a distance from the crater, and finding its way to the lower country, and even to the sea. The dimensions of this enormous gulf have been more accurately ascertained by Mr. Goodrich and Mr. Chamberlain, who have made a recent visit to it. By actual measurement they found the upper edge of the crater to be seven miles and a half in circumference, and at the depth of five hundred feet they satisfied themselves that its circumference was at least five miles and a half. They judged the depth to be one thousand feet.

Another crater of less size, not far distant from the great one, was seen by the deputation, and with their glasses they could perceive several extinguished craters upon the sides of Mouna Roa. The whole region is volcanic; and the lava, in numerous places, exhibits itself in most fantastic shapes, rising into cliffs, forming caverns, and presenting a precipitous, rugged, and variegated surface, all along the coast. No primitive formations seem yet to have been found on this island, nor any one of the group; and it is more than probable that all these islands have, in former ages, been raised from the sea by internal convulsions, and that the great mountains have been gradually formed by the accumulated products of volcanic eruptions, which have now ceased to exist, except in the craters above described, which serve as vents to the yet active, but subsiding, furnace beneath. The natives speak of earthquakes, and relate fearful traditions of the vengeance of the gods thus exercised. The soil consists of decomposed lava. The height of Mouna Roa has never been accurately measured, but variously estimated from 16,000 to 18,000 feet, being thus 1,000 or 2,000 feet higher than Mont Blanc, and 5,000 or 6000 higher than the Peak of Teneriffe. Mouna Kea, another mountain in the north-east part of the island, is nearly as high as Mouna Roa.—*N. American Review*, xxii. 334.

2. *Aërial Phenomena on the top of the Pic du Midi*.—M. Ramond has lately read to the Academy of Sciences at Paris, the conclusion of his memoir on the meteorology of the Pic du Midi. He has established the important fact, that while the wind at the foot of the mountain blows in all directions, at its summit it is constantly southerly; and that this southerly current is that which the earth's motion produces in the higher regions of the atmosphere, from the equator to the poles.

One day M. Ramond was witness, on the Pic du Midi, of a singular spectacle: his own shadow, and the shadows of two per-

sons who were with him, were thrown on a cloud a little distance above them, with a surprising exactness and sharpness of outline; and, which was more extraordinary, these shadows were surrounded by resplendent glories of the most brilliant hues. "A beholder of this magnificent sight," M. Ramond observes, "might fancy himself present at his own apotheosis." This effect has been observed by Bouguer, Saussure's sons, and others; and Bouguer explains the glory by the decomposition of light through frozen particles suspended in the cloud; but M. Ramond rejects this explanation, because the slightly elevated cloud on which the shadows appeared could not, he thinks, from the temperature prevalent on the Pic, hold in suspension any frozen particles.

The extreme transparency of the air in these elevated regions, causes several effects different from those observable upon the surface of the earth. Thus the heat of the soil, which absorbs the solar rays, is frequently, upon those heights, out of all proportion to that of the atmosphere. Thus again, the assembled rays at the focus of a lens, have much greater power than if they had passed through a gross and less-transparent air. M. Ramond remarked, that a lens of a very small diameter was sufficient to set fire to a body, which another lens, of double its size, could scarcely have heated in low places. The extreme brilliancy of colours on the summit of lofty mountains, induces M. Ramond to think that it might perhaps be easy to prove there the elevation of temperature produced by the different rays of the solar spectrum. It may be imagined that what prevents this from being proved with facility in lower places is, that the gross air which is found there is itself susceptible, in consequence of its want of transparency, of being heated in such a manner as to make the difference of the rays inappreciable.—*N. Month. Mag.*, xviii. 333.

3. *Power of the Sun's Rays.*—Mr. Mackintosh, an intelligent gentleman, who is contractor for the Government works carrying on at Stonehouse Point, Plymouth, and descended in the diving-bell with workmen, for the purpose of laying a foundation for a sea-wall, reports, that when the machine, which is provided with convex glasses in the upper part of the bell, was twenty-five feet under water, to his astonishment he perceived one of the workmen's caps smoking; on examining it, he found that the rays of the sun had converged through the glass, and burnt a hole in the cap; also, that similar effects had during hot weather frequently occurred on their clothes, so that the workmen, now aware of the cause, place themselves out of the focal point.—*Gentleman's Magazine*, 1826, 160.

4. *Transparency of the Ocean.*—Experiments were made during the voyage of the *Coquille*, to ascertain at what depth in the sea

an apparatus became invisible, composed of a plank two feet in diameter painted white, and weighted, so that on descending it should always remain horizontal. The results varied very much : at Offale, in the island of Waigiou, on the 13th of September, the disc disappeared at the depth of 59 feet ; the weather calm and cloudy ;—on the 14th, the sky being clear, it disappeared at the depth of 75.3 feet ;—at Port Jackson, on the 12th and 13th of February, it was not visible at more than 38.3 feet in a dead calm ;—the mean at New Zealand, in April, was 3.28 feet less ;—at the isle of Ascension, in January, under favourable circumstances, the extreme limits in eleven experiments were 28 and 36 feet.

5. *On the Native Locality of Platina*, by M. Boussingault.—This account is contained in a letter addressed to M. Humboldt, dated Bogota, 18th April, 1826 :—" I have just returned to Bogota, after passing six months in traversing the province of Antioquia. I have written to you several times, and my last letter was dated from Medellin. Since then I have visited the gold-mines of Santa-Rosa, which have presented me the interesting fact of the native locality of platina.

" Santa-Rosa de Osos is about ten leagues to the N.E. of Medellin ; it is a large village placed on a plain so elevated as to govern all the neighbouring country ; I have found its elevation above the sea by barometric observation to be 2775 metres (9104½ feet) ; at mid-day, barometer 21.43 inches, thermometer 59°.9 F. It is, I believe, one of the most elevated inhabited places of the province of Antioquia. The meridian height of the sun, taken on the 19th of December, gave me 6° 37' 43" for the latitude of the place. During my stay there, I have taken some sets of distances between the Moon and Aldebaran, but have not yet had time to calculate them.

" The rock of Santa-Rosa is a decomposed syenite, which is connected with the same rock, in an undecomposed state, which forms the fine and fertile valley of Medellin. On passing from this village to Santa-Rosa, it is first observed in the neighbourhood of San Pedro. Its alteration is very remarkable, the feldspar is changed into kaolin, and the amphibole (hornblende) has undergone an analogous alteration, having become an amphibole kaolin, if I may so express myself ; it is generally red, though sometimes yellow. Notwithstanding the state of decomposition in which the syenite of the Valle de Osos is found, it is always in place, and even preserves its regular stratification. All the gold-mines which are worked near Santa-Rosa are in this rock. Some of them are nothing more than washings of sands, but generally the gold is extracted from the auriferous veins. These veins are very numerous in the altered syenite ; they contain hydrated oxides of iron (*pacos*) mixed with quartz or yellow clay, called by the miners

azufre. The width of the veins is generally only a few inches, their direction very variable, and they are generally vertical. The gold is found disseminated in the *pacos*, and in the quartz and clay which accompany these substances. All the mines and works at Santa-Rosa are open to the sky, and the method is the same as that followed at Titiribi. After having removed the vegetable earth, and laid the *heads* of the veins open, an abundant current of water is conducted on to them, which by the inclination given to the worked surface traverses it with extraordinary rapidity. Men armed with iron levers (*varas*) are placed in the current, and detach the masses of rocks, which are immediately carried away by the force of the water; other workmen break the larger pieces by blows, so as to render them easily transportable by the waters. The debris of the rock thus produced is carried by the water into a long narrow canal made at the bottom of the works, and which, having but little inclination, allows the motion of the water to become less rapid, and it therefore deposits the divided rock and the grains of gold, and carries off only the finest earthy particles. When iron and water have thus worked for many days, and produced a deposit of auriferous alluvium in the canal, the washing is proceeded to in the usual manner, with this difference, that the washer is careful to put on one side all the pieces of oxide of iron (*paco*) which he finds in his wooden trough (*batea*); these *pacos* are put with those collected directly from the canal, all are broken on stones, and washed; the quantity of gold which they yield is considerable.

"It is in the gold in powder coming from one of these veins that I found the *grains of platina*. These grains were similar in their form and aspect to those procured from Choco. This fact, of the locality of platina in a vein of oxide of iron, appears to me as if it would throw some light on the origin of the platina found in alluvial countries, and which has been thus far very problematic. The form of rounded plates, in which the platina of Choco has occurred, with its situation, has induced the supposition that the metal had been subjected to friction for a long time. It is, therefore, very remarkable that the platina of Santa-Rosa, disengaged from its gangue, as it were, under my eyes, should have this form. The rolled appearance, however, is not peculiar to the platina, but is frequently observed also on the gold obtained from the *pacos*. This is a fact which I had frequent opportunities of verifying during my prolonged stay in the province of Antioquia. When at the gold mines of Buritica, I was enabled to ascertain that the syenitic and porphyritic greenstone of that province continued to Choco: for, in going from Buritica to Cañas Gordas, near the Cerro de Morroacho, we pass the *Alto de Toyo*, which I found rose to a height of 2696 metres (8845½ feet); this plain is part of a little cordillera which separates the waters going to

Cauca from those proceeding to Atrato. At Cañas Gordas, I dwelt amongst the Chocoe Indians. The rock at Cañas Gordas is the same with that of Buritica, a compact greenstone, which near Antioquia alternates with syenite in small grains. In certain localities these grains are more developed, the crystals of feldspar become visible, and the rock passes into a porphyritic greenstone."

—*Ann. de Chimie*, xxxvi. 209.

On this letter M. Humboldt remarks, that "the discovery of M. Boussingault accords well with the geognostic relations generally presented by the syenitic and greenstone formations. The syenites of Norway, Greenland, and Germany abound in zircons and titaniferous iron, and these substances are constantly mixed with the platiniferous sands of Choco. The zircons appear to be wanting in the platiniferous alluvium of Brazil, which seems due to the decomposition of an intermediate quartz-formation mixed with oxide of iron; but Brazil presents the remarkable union of diamonds with platina and palladium. On the plain of Minas-Geraes, the chloritic quartz (itacolumite) attains a thickness of more than 1000 feet, and superposed, not on mica-slate, but constantly on thonschiefer, is covered by a ferruginous breccia (itabirite) extremely auriferous. When washed, gold, platina, palladium, and diamonds are found together at Corrego das Lagens; gold and diamonds only at Tepico; platina and diamonds only at Rio Abaeté. The decomposed chlorite schist, from which is obtained topazes, cyanite, and the euclases of Brazil, belongs, with the itacolumite and itabirite, to the same type of transition-formation. Diamonds have been observed inclosed in ferruginous quartzose conglomerates, which form small superficial layers, and in the itabirite. These analogies, well worthy of fixing the attention of geologists, may allow the presumption, that, in Brazil, gold, platina, euclase, cyanite, diamond, and sulphur, have simultaneously their primitive locality in the chloritic quartz and the accompanying strata; there is there neither syenites with zircon, nor greenstone amphibolic rocks, which at Choco and at Antioquia contain the platiniferous veins. It appears to me very probable that the platina of Western America does not belong to the same rock-formation as the platina of Eastern America, &c.—*Annales de Chimie*, xxxii. 207.

6. *Rattle-snakes*.—Mr. Neale, it is said, has succeeded in America in taming rattle-snakes, by means of music, so as to prevent them from doing harm. He asserts that they really possess the power of enchanting animals, or of rendering them motionless through terror, having seen examples even in his garden. The effluvia of these animals has nothing nauseous in it.—*New Edin. Phil. Journal*.

There is a person now in London * who has collected together

* Or was, when this sheet went to press.

as many as fifty of these dangerous animals; they appear to be in excellent health, are perfectly good-tempered towards each other, very clean, and in a morning when first disturbed, by being looked at, make so much noise with their rattles, that persons in conversation in the room can scarcely hear each other speak. They are slow, but very graceful, in their motions and attitudes. When thus confined, rats, mice, and other small animals put into the boxes to them are much alarmed, dash about, and one rat even bit the snakes. The latter, if observed, will frequently leave the animal untouched and uninjured, notwithstanding its restlessness, for an hour or two; but upon the absence of the observers will generally strike and kill it immediately, whether they want food or not.

7. *Analyses of Elastic Bitumens.*—These analyses were made by M. Henry, *fils*. The English specimen was from Derbyshire, in brown masses, slightly translucent, and of a greenish colour when viewed by transmission; it was soft, elastic, burnt with a white flame, and bituminous odour. Its specific gravity varied from 0.9 to 1.23. The French bitumen resembled the preceding, but was opaque, and floated upon water. It was discovered in 1816 at the coal-mines of Montrelais.

	English.	French.
Carbon	.5225	.5826
Hydrogen	.0749	.0489
Nitrogen	.0015	.0010
Oxygen	.4011	.3675
	1.0000	1.0000

Ann. des Mines, xii. 269.

8. *Morbid Alterations of the Blood.*—M. Segalas lately read, at the Academy de Médecine, a memoir on the blood, which he considers to be susceptible of morbid alterations during life, thus becoming a frequent cause of disease. The following experimental facts are adduced as proofs of this opinion:—1. That concentrated alcohol acts chemically on the blood during life.—2. Diluted alcohol injected into the veins, or bronchia, produces immediate intoxication; the same effect is induced when it is applied to other parts, but more slowly.—3. The effects of alcohol, introduced into any other part than the veins, are in their intensity and quickness, in direct relation with the absorbing powers of that part, and are independent of the nerves distributed there, especially of those of the stomach.—4. That the effects are hastened, increased, or retarded and diminished, by the circumstances which favour or impede the entrance of alcohol into the blood.—5. That inebriation vanishes as alcohol leaves the blood, and this effect depends on the circumstances being more or less favourable

to exhalation.—6. That the effects of alcohol are in relation, not with the quantity introduced into the organs, but with the quantity in the blood.—7. That profound intoxication, and death by drunkenness, are attended with an evident alteration of the blood, and some changes of the solids less evident.

M. Segalas infers from the above theory, that oil and ammonia are useful in drunkenness ; perhaps they exert an action on the blood opposed to that of alcohol. (Archives Générale.)—*Med. Journ.* lvi. 280.

9. *Tenacity of Vegetable Life*.—An instance of this kind occurred some time since in the Royal Park of Bushy. Some small portion of it was broken up for the purpose of ornamental culture, when immediately several flowers sprung up of the kinds which are ordinarily cultivated in gardens ; this led to an investigation, and it was ascertained that this identical plot had been used as a garden not later than the time of Oliver Cromwele, more than 150 years before.—*Mon. Mag.* ii. 74.

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